SCIENCE

A WEEKLY JOURNAL DEVOTED TO THE ADVANCEMENT OF SCIENCE, PUBLISHING THE OFFICIAL NOTICES AND PROCEEDINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

FRIDAY, FEBRUARY 2, 1906.

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MSS. intended for publication and books, etc., intended for review should be sent to the Editor of SCIENCE, Garrison-on-Hudson, N. Y.

THE PARTITION OF ENERGY.[1]

As I rise in this place to address you, I recall a dear friend, who stood before you in a similar position three years ago, and whose premature death has dealt so severe a blow to this association and to the science represented in this section. The name of DeWitt Bristol Brace will always be honorably remembered in the history of physics. While a student at Boston University, he began the study of that science, and after his graduation in 1881, he proceeded to Johns Hopkins University to devote himself exclusively to it. After two years of study there, he went to Berlin, where he heard the lectures of Kirchhoff, and worked in the physical laboratory under the direction of Helmholtz. It was in Berlin that he definitely settled the whole course of his subsequent scientific career, by insisting on taking up, as his subject of research, the difficult question of the exact mode of transmission of a polarized ray which is undergoing magnetic rotation. This question was out of the line along which the work of the director and of his students was proceeding at that time, and Brace not only set the problem for himself, but owed entirely to his own inventive genius the brilliant method which he proposed for its solution. I remember how difficult it was for Brace to convince our director of the possibility of transmitting the ordinary and extraordinary beams in a common direction in a crystal of Ice-

[1] Address of the vice-president and chairman of Section B—Physics, American Association for the Advancement of Science, New Orleans, 1905.

land spar; and the doubts of the director of the realization of the demonstrated conditions, even if they could be proved possible in theory. After Brace had succeeded in experimentally obtaining the conditions which his theory called for, he applied them to the examination of the question of the transmission of a polarized ray in a magnetic field. He was not able at that time to answer the question which he had in mind, but he was able to produce a memoir full of discoveries and ingenious suggestions, which culminated many years afterward in his final resolution of the problem, by the actual separation of the plane polarized ray in a magnetic field into two circularly polarized components.

Brace took his doctor's degree in 1885. After a short service at the University of Michigan, he became, in 1888, professor of physics at the University of Nebraska, where he devoted himself, with the conscientious fidelity which was a part of his nature, to the duties of his position. These duties he did not interpret in any narrow sense. He conceived that it was a large part of the obligation, which lay on him, to promote the progress of physics, not only by his own researches, but by organizing and assisting a corps of fellow workers. By his enthusiastic zeal, and by the wise administration of the facilities afforded him, he was able to draw around him a number of men who caught inspiration from his spirit, and who have, by their published researches, for several years past, been giving to the institution in which they worked an honorable prominence.

In the midst of his official duties Brace gave what time and strength he could spare to his own investigations. His mind was exceedingly fertile in the invention of methods of observation. The subject of optics, in which his first original work had been done, remained the subject of his choice throughout his life. As that science has developed in connection with certain theories of electricity, fundamental questions have arisen, principally relating to the general subject of the relative motion of matter and the ether, which can only be answered by carrying observation to a degree of refinement which was not attained in the classical experiments of the great French and English investigators. Brace was profoundly interested in this great subject, and his principal task, for several years, was to improve the methods of observation to such a degree that indubitable conclusions might be drawn in regard to the results of experiments left doubtful by earlier investigators. By the union of inventive power with great experimental skill he succeeded in extending the range of observation in his repetitions of the experiment of Lord Rayleigh, to test for possible double refraction in an isotropic medium; the experiment of Mascart, to test for a differential effect on the natural rotation of the plane of polarization; and the experiment of Fizeau, to test for a possible change in the azimuth of the plane of polarization of a ray, polarized by refraction, to so high a degree of accuracy as to be able to confirm with confidence the negative results of Rayleigh and Mascart, and to assert with equal confidence that the positive result, which Fizeau believed he had obtained, must have been due to some experimental error and not to the cause to which it is sometimes ascribed. He had in mind other experiments to test the same general question, and we who knew the rare combination of powers which he possessed did not even despair of his succeeding in making the final test which is necessary, by the determination of the velocity of light when transmitted in one direction only.

This is not the place to enumerate Brace's contributions to science. It is sufficient to notice that part of his work in which his activity culminated, and which

was most closely connected with his hopes for the future. All his other work showed the same distinguished qualities; unusual ability of invention, great experimental skill, and the faculty of choosing central subjects to which to devote his powers.

Brace was for several years a member of this association, and was vice-president and chairman of Section B for the year 1901–2. He was also a member and vice-president of the Physical Society, and considering the distance between his home and the usual place of meeting of that society, a frequent attendant at its meetings. He very greatly enjoyed the opportunities afforded by them to meet with his friends. Under a somewhat retiring manner he concealed a warm heart and a sincere and unselfish admiration of other men's abilities and successes. His encouragement and sympathy were always given to those who came to him to talk about their scientific projects; and it renews my own sense of bereavement when I recall the cordial way in which he discussed with me the subject of the present address. His life of untiring devotion to duty and of genuine goodness will always remain a sweet memory to his friends; as his lofty and unselfish zeal for the promotion of knowledge will be an example and a stimulus to those who know the story of his work.

In the retrospect of the progress of physics during the past year, the attention is first and most forcibly attracted to the work which has been done in the general subject of radioactivity. By the continued labors of J. J. Thomson and of those in his immediate circle of fellow-workers, of Rutherford and Bragg, of Lenard and Kauffmann, and of many others, our knowledge of the details of the radioactive process has been very much extended. Some progress, though not so great, has been made in the development of the theory. But the main result, which, it seems to me, can be considered as attained during the year, is the final appreciation of what we may call the electrical theory of matter, or at least of the possibility of explaining the properties of matter, even those of the most fundamental character, by the laws of electric action. Of course such an explanation can not yet be given, but it seems to me that, so far as we can judge by the hints and the *obiter dicta* which appear from time to time, the belief in the ultimate possibility of such an explanation has at last become general.

I doubt if the history of science affords another instance of so fundamental a revolution in modes of thinking, occurring in so short a time. For it is only ten years ago that Röntgen discovered the peculiar radiation that is known by his name, and I think we may fairly date the beginning of the study of radioactivity from that important discovery. On the one hand, the facility with which gases can be ionized by the Röntgen rays aided J. J. Thomson materially in his study of ionization, which led to his discovery of the electron and his investigation of its properties. On the other hand, the fluorescent effects of the Röntgen rays incited Becquerel to his examination of uranium, and led to his discovery of its radioactive properties; and from this to the isolation of radium by M. and Mme. Curie, and to the splendid series of researches which have been carried out on that substance and on other allied substances, the way is clear. Röntgen's discovery was made only ten years ago, and it is a marvelous illustration of the large sum of human effort applied to the study of science, and of the active, I might almost say the radical, tendency of the modern mind, that in so short a time we have obtained so great a body of established knowledge, and experienced so complete a revolution in our most fundamental modes of scientific thought.

In the domain of optics, also, there has been considerable progress, of which a very important part is due to our lámented friend, whose work I have already considered. In other parts of our science, the work which has been done is not of so consecutive a character, nor has it been productive of such important results as to call for particular mention.

The subject upon which I wish to speak to you to-day is that of the partition of energy. As is well understood, the energy here referred to is the kinetic energy of the moving particles, which, according to the kinetic theory of matter, constitute a body. The general theorem which I wish to discuss may be stated by saying that the kinetic energy of the body is so distributed among the degrees of freedom, by which the state of the body as a dynamical system is described, that an equal share is, on the average, allotted to each degree of freedom of each type of molecule.

Since the enunciation of this theorem as applied to gases, by Maxwell, in 1859, it has from time to time attracted the attention of the mathematical physicists. Lately it has again been brought forward, and the difficulties which surround it very considerably removed, by the work of Jeans. This author has collected the results of his own researches, in combination with a historical and critical study of previous work on the question, in a recently published book, which covers the ground so completely as to supersede any independent study of the subject which I could have made; but I trust that the exposition of it which I shall give will be of interest as an introduction to the experimental matter which I shall adduce; and that this will at least indicate a way in which we may hope to obtain some confirmation of the theorem of equipartition.

The questions which have always been raised about this important theorem of the kinetic theory at once come to our minds. First, is the theorem true, or rather, does it state what would be true for an ideal system of particles moving freely within a containing vessel? second, is the proof of the theorem impeccable? third, is there any experimental evidence that it applies to real bodies?

I would remark about the first question that the theorem is so distinguished by its simplicity, and by its aspect as a sort of unifying principle in nature, that few men can set it fairly before their minds without at least desiring to believe it true. Most of those who have recognized that Maxwell's original demonstration was not flawless are still convinced of the truth of his conclusion, or at least believe his conclusion to be so probable as to make it worth while to try for a more accurate demonstration. Their state of mind is like that of Clausius and of Lord Kelvin, when they perceived that Carnot's theorem respecting the efficiency of a reversible engine could not be proved in the way in which Carnot tried to prove it.

With respect to the second question, it was very soon pointed out that Maxwell had made in his proof an assumption that could not be justified by immediate inspection, and which was itself in need of demonstration or of avoidance. The later demonstrations of Maxwell and Boltzmann have been likewise subjected to criticism, and can be shown to involve assumptions that will not be granted on inspection. The difficulties that arise in these proofs come from the necessity of applying in them the calculus of probabilities, and center around the question of the legitimacy of the application of that calculus. It is commonly agreed that Maxwell and Boltzmann have assumed a condition of the system of moving particles, as a requisite for the application of the calculus of probabilities, which is contradicted by many systems of which

we have certain knowledge, and can not without proof be admitted as likely to obtain in other systems, about which less is known. In the method employed by Jeans the application of the calculus of probabilities is made in a different manner, and does not necessitate the introduction of the hypothesis of Maxwell and Boltzmann. It seems to me that, in this last form of the theory, the difficulties which have environed the subject have at last been mastered.

In respect to the third question, that concerning the experimental evidence for the truth of the theorem, it is well known that, in general, Boyle's law follows as a consequence of the general principles of the kinetic theory, that Gay-Lussac's law is an immediate consequence of a relation plausibly assumed between temperature and the kinetic energy of the molecule, that the motion of the radiometer and the laws of transpiration and many other properties of gases can be deduced from the general theory; and, in particular, that Avogadro's law follows from the simplest form of the theorem of equipartition. But further proof of this theorem in its general form is still needed. Such proof as we have will be discussed later in this address.

Let us consider a little more fully the proofs of the theorem of equipartition. It was first enunciated in 1845, in a paper presented by Waterston to the Royal Society, but this paper was not published at that time, and Maxwell's paper of 1859 first brought the theorem to the attention of the scientific world. In that paper Maxwell undertook to prove that when two systems of molecules move in the same vessel the mean *vis viva* of each particle will become the same in the two systems.

The proof of this proposition by Maxwell is the one still commonly employed in elementary expositions of the kinetic theory of gases. As applied first to a single gas, he considers the molecules or particles of the gas as elastic spheres, and represents the average number of particles which have a velocity in one direction lying between two very near limits as a function of that velocity. To represent the average number of particles which have a velocity in either of the other two rectangular directions lying between certain near limits, he uses the same function, and he then supposes that the three velocities thus used are independent of each other, so that the average number of particles which possess all three velocities at once will be given by the product of three independent probabilities. Since this number depends only on the relative motions of the particles, and not on the particular directions in which the coordinate axes have been drawn, it may also be represented by a function of the resultant velocity of the particles, or by a function of the sum of the squares of the component velocities. Equating these two expressions, a functional equation is obtained, the solution of which leads to the well-known exponential law of the distribution of velocities among the molecules.

By extending the method just described to the consideration of the relative motion of the particles of two gases, Maxwell proceeded to show that the probable number of particles, whose velocities differ by a certain amount, is expressed by the same exponential function as that already obtained; and he shows further that the probable mean relative velocity is the square root of the sum of the squares of the mean velocities in the two systems.

On the basis of this proposition, Maxwell proves that the average kinetic energy of the molecules of two or more gases, when they are mixed, will be the same for each. To do this he shows simply that the difference of the mean kinetic energies of the molecules of two gases will be diminished by collision, so that it is only neces-

sary that a sufficient number of collisions take place to reduce this difference to zero.

The defect of this demonstration lies in the assumption that the velocities in the three rectangular directions can be considered as independent. As they do not enter independently in the equations of collision between the molecules, we might fairly expect them to be related to each other, until they are proved to be independent.

I have dwelt on this first method of Maxwell's, because of its historic importance, and because it illustrates the difficulty of deciding by inspection on the conditions which may legitimately be assumed for the application of the calculus of probabilities. In the elaborate method subsequently developed by Maxwell and by Boltzmann, the same difficulty is met with in another form. In this method the molecules are considered as spheres, freely moving about within a vessel and colliding with each other. The effort is made to determine the characteristics of the motion of the assemblage of molecules which must obtain if the condition of the assemblage is to be, to outside inspection, uniform. To do this, we consider a number of molecules belonging to a certain class characterized by possessing certain component velocities before collision, and certain other component velocities after collision, and a number of molecules belonging to another class characterized by possessing the same component velocities in the reverse order, and, considering the probable number in each of these classes as a function of the component velocities, we write down the expression for the probable number of molecules of these two classes which occupy the same element of volume and so are in collision. With this expression we can obtain an expression for the average increase in the number of molecules in one of the classes, due to collisions, and this ought to be zero if the condition of the assemblage is to be uniform. From the discussion of this last expression follow Boltzmann's H-theorem, the formula for distribution and the theorem of equipartition.

When the mode is analyzed in which the expressions giving the probable numbers of molecules in the two classes are combined, it appears that an assumption is involved in it which is not evident on inspection: namely, that the probability of the presence of a molecule of one class in an element of volume is independent of the coordinates of that element, or, what amounts to the same thing, of the probability of the presence, in the same element of volume, of a molecule of the other class. The gas to which this assumption applies is called by Boltzmann unordered with respect to the distribution of the molecules (*molekular-ungeordnet*). Jeans uses for this condition of a gas the very convenient phrase 'molecular chaos.'

Now we evidently can not assert off-hand that this chaotic condition will obtain in all systems of particles which represent real gases. When Boltzmann tries to define it he does so by negative instances, showing what the condition of the system might be which would be 'ordered' with respect to the distribution of the molecules; and then leaves us to infer that the vast majority of distributions do not possess any peculiarities which would put them in the 'ordered' class. But this inference is not readily drawn. It is evident that any condition of the system is ordered, in the sense that the successive conditions follow on mechanical principles from the initial condition. All that we can do by inspection is to cherish the hope that, while all systems are 'ordered' in this sense, yet a vast number of them—an infinite number of them, it may be, in comparison with those 'ordered' in Boltzmann's sense—are still 'unordered' in such a sense that the

application of the calculus of probabilities to them, as made by Maxwell, may not lead to an erroneous result. To assert that this is so requires proof.

An alternative method of dealing with the theory of gases, of which the latest development is due to Jeans, proceeds by treating a gas as a single dynamical system, specified by the positional coordinates and the component velocities of its molecules. We then, to use Jeans's words, consider 'an infinite number of systems, starting from every conceivable configuration, and moving over every path; and investigate, as far as possible, the motion of this series of systems, in the hope of finding features common to all.' Jeans carries out this investigation by representing each particular phase of the system by a point in a generalized space. The successive phases through which one of the systems will pass will be represented by the points along a line in the generalized space. Jeans shows that these lines will be 'stream lines' in the space; and that to investigate the infinite number of systems already supposed comes to the same thing as to suppose the generalized space filled with a fluid, moving along stream lines determined by the dynamical equations of the gas, and to investigate the motion of this fluid. This motion is found to be a 'steady motion.' The advantage of this mode of procedure is that the applications of the calculus of probabilities are made to the elements of the generalized space, and are obviously legitimate.

By an argument based on this fundamental mode of representation Jeans shows that all but a negligibly small fraction of the generalized space represents systems in which the density of the gas is uniform; and that within that part of the generalized space which represents states of the system in which the energy is constant, all but an infinitely small fraction represents systems in which the velocities are distributed according to Maxwell's exponential law. Such a state of the system Jeans calls the 'normal state,' and his conclusion is that it is infinitely probable that a gas in ordinary circumstances will be in the normal state.

We may consider this result as an *a posteriori* proof of the hypothesis of molecular chaos.

By a treatment that is essentially similar, and without using the hypothesis of molecular chaos, it follows that it is infinitely probable that the energy of the gas is, on the average, distributed equally among the degrees of freedom corresponding to molecules of different types, and also among the degrees of freedom of each type considered independently.

When Maxwell attempted to prove the law of equipartition for the time averages of the energy associated with the different degrees of freedom of a single system, he introduced the hypothesis that the system goes through all possible phases, consistent with the conservation of energy, before returning to its initial phase. It is difficult to see how this can be the case in a system self-contained and entirely subject to the laws of dynamics, such as, for example, a gas within the smooth envelope ordinarily postulated in the theory. But if we think of the envelope as itself an assemblage of moving molecules, it may be that the courses of the gas molecules will be changed so irregularly by impacts at the boundaries that the gas may be thought of as experiencing so many fortuitous disturbances that it will practically fulfil the condition of passing through all possible phases. In this case the law of equipartition can be extended to the time average of any one degree of freedom.

Lord Kelvin has taken exception to this form of the theorem of equipartition, asserting that the proof is invalid, and that

it is not true in fact. To demonstrate the latter statement, in default of any direct experimental method, he has resorted to the calculation of the paths and velocities of a moving body within an envelope of some assumed form, and a comparison of the kinetic energies associated with each degree of freedom. To introduce the element of chance, the courses of the body were mapped out by the help of numbers obtained by drawing numbered cards from a pack. The results obtained differed considerably from the exact equalities deduced from the Maxwell-Boltzmann theorem. I do not pretend to be able to show that these results of Lord Kelvin are of no value as evidence against the truth of the theorem, but I would remark that we can at least justify a doubt about them by noticing how small a deviation in the experiments from perfect impartiality of conditions will suffice to produce a large deviation from the expectation of the theory of probabilities. To test this, and remembering that when I used to play whist we noticed that a black card turned up as trump oftener than a red one, I procured a new pack of cards, which ought to be impartial and unbiased, if anything is, and cut it a number of times, noting the suit of the card exposed by the cut. In 312 cuts the black suits were recorded 196 times, the red suits 116 times. Spades were recorded 111 times, clubs 85 times, diamonds 68 times, hearts 48 times. Practically the same ratio (184–121) between the black and red suits was obtained with an old pack, though the order of suits was different. Such a persistent departure from the expectation of an equal number of each color and of each suit indicates that for some reason the cards are not impartial; and a scrutiny of a new pack shows, I think, the reason for this. When the pack is examined, the bottom card is usually the ace of spades, and then the spade suit follows in order. The uppermost suit is generally the hearts. I believe that, when the pack is trimmed, the knife is pressed out as it goes down, so that the upper cards are cut a little smaller than the lower ones. The difference in size can be seen if the pack is evened up on a smooth surface. Some cards will then appear a little wider than the others, and if they are picked out, they will generally be found to be black cards. Now I do not know how Lord Kelvin's pack of cards was made, or how the cards were drawn, but I think we may fairly suppose that the discrepancies of 15 per cent. or so, which appeared in his experiments, may have been due, not to a failure of the theorem of equipartition, but to trifling departures from impartiality in his method of experimentation.

We are now ready for the examination of the experimental evidence for the applicability of the theorem of equipartition to real bodies. The most important evidence that bears on the question is found in the observed values of the specific heats of gases, and of the ratios of their specific heats of constant pressure and of constant volume. If we consider the distribution of the energy which enters a gas at constant volume when its temperature rises one degree, designating its specific heat of constant volume by C_v, the increase in the energy of translation of the molecules by C_o, and the ratio of the two specific heats by γ, it is easy to show that

$$\gamma - 1 = \frac{2}{3}\frac{C_o}{C_v}.$$

Now if each degree of freedom acquires an equal share of kinetic energy, say k, the energy of translation increases by $3k$, so that

$$\gamma - 1 = \frac{2k}{C_v}.$$

Furthermore, C_v will equal k times the total number, n, of degrees of freedom of

the molecule, added to an unknown amount, P, of potential energy. We thus get

$$\gamma = \frac{2k}{nk + P} + 1.$$

If we assume that the molecules of the gas do not take up potential energy, so that $P = 0$, and then assume that the molecules are practically points, so that $n = 3$, corresponding to the three translational degrees of freedom, we have $\gamma = 5/3$. This is the number found for this ratio in certain cases, in one of which, at least, that of mercury vapor, we have independent reasons to believe that the molecule is monatomic. If we set $n = 5$, as would be the case if the molecules were solids of revolution, we get $\gamma = 7/5$, which is the value found for several diatomic gases. If $n = 6$, as would be the case if the molecules were irregular solids, we get $\gamma = 4/3$, a number not often found as the value of the ratio, the numbers obtained for gases not belonging to the other two classes being generally less than this. So far we seem to have an imperfect agreement with the theory, but the conditions assumed are evidently not those of real molecules. Staigmüller has shown a way, to which I shall direct particular attention, to modify the formula so that it can be applied to real gases; but before doing so, I wish to consider the general question, which will not be settled by our being able to find that certain assumed values of n will give observed values of γ. Have we a right to believe that the number of degrees of freedom of a molecule, other than the three degrees of translational freedom, are ever, in any case, so few as we must suppose to get the results referred to? Are we not rather bound to believe, from the evidence of internal vibration afforded us by the spectroscope, that the molecules or the atoms of all gases are vibrating in many modes, or are compound bodies whose parts are executing vibrations? And should we not therefore set our number n of degrees of freedom very large, and so obtain a value of γ practically equal to unity for all gases, in entire disagreement with the experimental results? The view of the constitution of the atom which prevails at present, that it consists, at least in part, of an assemblage of electrons having the essential properties of mass and moving in orbits with enormous velocities, supports the evidence of the spectroscope, and makes it all the more necessary for us to admit that the molecule of gas will have a great number of degrees of freedom.

A reconciliation of these views with a modified doctrine of equipartition has been made by Jeans. The proofs of the theorem of equipartition apply to a conservative system, and fail as soon as they are applied to a system in which the energy is not constant. Now we know that, as a matter of fact, every material system is transferring energy to the ether—indeed, as Jeans remarks, our seeing it at all depends upon that operation—and it appears therefore that we ought not to expect the theorem of equipartition to apply to real systems without further examination. To carry on this examination we conceive of a system so constituted that the energy which corresponds to certain of its degrees of freedom can and does pass rapidly into the ether, while that corresponding to the remaining degrees of freedom is dissipated by transfer to the degrees of freedom of the first class. On this supposition Jeans shows that the energy resident in the system may be divided among the degrees of freedom according to a modified mode of equipartition; those coordinates whose energy is not transferred directly to the ether possessing equal amounts of energy, corresponding to the temperature of the body indicated by the thermometer and to the formula $mv^2 = 3RT$, while those coordinates whose

energy is directly dissipated into the ether also possess equal amounts of energy, different from the other amounts, and corresponding to another function t, whose dimensions are those of temperature, and which conforms to a formula similar to the one just given, with the same constant R. The two temperatures thus introduced Jeans calls the principal and subsidiary temperatures, and the degrees of freedom to which they correspond, the principal and vibratory degrees of freedom. He shows that, provided the product of the time occupied by a collision between two molecules and the frequency of the atomic vibration is large, the transfer of energy from the principal to the vibratory degrees of freedom goes on very slowly, and he shows further that we may believe that the postulate here made applies to real bodies; so that, when a gas is heated, we may consider that practically all the energy which it receives is taken up by the principal degrees of freedom. If this be granted, we may ignore, for practical purposes, the multitudinous vibratory degrees of freedom, and are brought back to the simpler view of the constitution of the molecule as a collection of atomic masses bound together into a system. We have thus set before us the task of making plausible estimates of the number of principal degrees of freedom in the various gases, and of calculating the specific heats of these gases and the values of the ratio of their two specific heats. In doing this we shall follow the procedure of Staigmüller.

In the formula

$$\gamma = \frac{2k}{nk + P} + 1$$

the denominator $nk + P$ represents the energy received by the molecule, when its temperature rises one degree. We assume that the potential energy P is entirely that due to the displacements of the atoms in the molecule, and that the motions of those atoms are simple harmonic, so that the mean potential energy corresponding to each internal degree of freedom is equal to the mean kinetic energy. Using a to represent the number of degrees of freedom of the molecule as a whole, and i to represent the number of internal degrees of freedom, we have

$$n = a + i, \quad P = ik, \quad \text{and} \quad \gamma = \frac{2}{a + 2i} + 1.$$

When we use θ to represent the sum $a + 2i$, the formula takes the simple form

$$\gamma = \frac{\vartheta + 2}{\vartheta}.$$

Our task is to estimate the values of a and i in particular cases, and to calculate therefrom the values of γ.

Before proceeding to do this we will calculate the expression for the specific heat of constant volume in terms of the degrees of freedom. Denoting again by k the energy corresponding to one degree of freedom, which a gram-molecule of the gas receives when its temperature is raised one degree, and by m the molecular weight, we may write $C_v m = k\theta$, and may calculate k as follows:

We know from Joule's calculation, that the velocity of mean square for hydrogen is 1.842×10^5 centimeters per second. From this we get the total mean kinetic energy of translation of a gram-molecule of hydrogen, one third of which is the amount of kinetic energy apportioned to one degree of freedom. We then get the change in the energy apportioned to one degree of freedom, which occurs when the temperature rises one degree, by dividing by the absolute temperature 273, and reducing the result to gram-degrees, obtain for k the value $k = 0.9886$. This value is so nearly equal to 1 that for many of our subsequent calculations k will be taken equal to 1.

We are now in position to calculate not only the values of γ for various gases, but also their molecular heats, that is, the heat capacities of their gram-molecules, from estimates made of the probable number of their principal degrees of freedom.

In the case of a monatomic gas we suppose the molecules to act either as points or as smooth spheres, for which the only effective degrees of freedom are the three translational ones. For these, therefore, we have

$$\vartheta = 3, \quad C_p m = 4.943, \quad \gamma = \tfrac{5}{3} = 1.66+.$$

I do not know that the molecular heats of the monatomic gases have been measured, but the values of γ obtained for mercury vapor, krypton, helium and argon range from 1.666 to 1.64.

For the diatomic gases, while we still treat the atoms as points or smooth spheres, we notice that 5 coordinates are necessary, being also sufficient, to determine the position and orientation of the molecule, in view of the fact that its orientation about the line joining the two atoms is indifferent. The only other principal degree of freedom which the molecule can possess is the single one which determines the distance between the atoms. If this distance is fixed, we have $i = 0$, and $\theta = 5$; if it is variable, $i = 1$, and $\theta = 7$. From these assumptions we can calculate the constants for comparison with the results of observation.

$\vartheta = 5$. $\kappa(\vartheta + 2) = 6\ 9202$. $(\vartheta + 2)/\vartheta = 1.400$.

		$C_p m$	γ
Hydrogen	H_2	6.809	1.402
Nitrogen	N_2	6.830	1.405
Oxygen	O_2	6.960	1.402
Nitrous oxide	NO	6.951	—
Hydrogen bromide	HBr	6.642	1.400

$\vartheta = 7$. $\kappa(\vartheta + 2) = 8.8974$. $(\vartheta + 2)/\vartheta = 1.286$.

Chlorine	Cl_2	8.811	1.33
Bromine	Br_2	8.88	1.293
Iodine	I_2	8.50	1.294

When we consider the triatomic molecules, we find several of them of the type of which the molecule of water vapor is an example, in which we may suppose that the bivalent atom stands between the other two similar atoms, at the center of gravity of the molecule. If we can conceive the atoms thus placed and take the coordinates not fixed by this assumption as corresponding to subsidiary degrees of freedom, we may consider the energy of the molecule as determined by the 5 degrees of freedom which fix the position and orientation of the molecule, and by one additional degree of freedom, determining the distance between either of the univalent atoms and the center of gravity. On these assumptions we obtain $\theta = 7$.

$\vartheta = 7$. $\kappa(\vartheta + 2) = 8.8974$. $(\vartheta + 2)/\vartheta = 1.286$.

		$C_p m$	γ
Water vapor	H_2O	8.649	1.29
Carbon dioxide	CO_2	8.91	1.285
Hydrogen sulphide	H_2S	8.33	1.276

For reasons which I will not stop to give, Staigmüller considers the molecule of bisulphide of carbon to be of a different class, in which the above described symmetry does not obtain, so that we have $a = 6$, and each of the sulphur atoms determined as to its distance from the center by one coordinate, so that $i = 2$, and $\theta = 10$.

$\vartheta = 10$. $\kappa(\vartheta + 2) = 11.863$. $(\vartheta + 2)/\vartheta = 1.200$.

		$C_p m$	γ
Carbon bisulphide	CS_2	12.12	1.197

For the two vapors, phosphorous chloride and arsenious chloride, we may think of the univalent atoms as free to move in planes at right angles to the directions of the valencies of the trivalent atom, each with 2 degrees of freedom, while 6 more are needed to determine the position and orientation of the molecule. We have thus, in these cases, $\theta = 18$.

$\vartheta = 18$. $\kappa(\vartheta + 2) = 19.77$. $(\vartheta + 2)/\vartheta = 1.111$.

		C_m	γ
Phosphorous chloride...	PCl_3	18.49	—
Arsenious chloride.....	$AsCl_3$	20.32	—

For the vapors silicon tetrachloride, titanium tetrachloride, and tin tetrachloride, we make the same assumptions as in the last case about the freedom of the chlorine atoms, and get $\theta = 22$.

$\vartheta = 22$. $\kappa(\vartheta + 2) = 23.726$. $(\vartheta + 2)/\vartheta = 1.091$.

		$C_p m$	γ
Silicon tetrachloride....	$SiCl_4$	22.47	—
Titanium tetrachloride.	$TiCl_4$	24.77	—
Tin tetrachloride	$SnCl_4$	24.41	—

There is a difficulty which I can not solve about the methane derivatives. Their specific heats have been determined only in two cases, but we have two sets of values of γ, one obtained by Müller, the other by Capstick. Müller's values conform fairly well to the hypothesis that the hydrogen atoms of the methane (CH_4) molecule are fixed, and that each chlorine atom substituted for a hydrogen atom introduces 2 interior coordinates.

	ϑ	$(\vartheta + 2)/\vartheta$	γ
CH_4.....	6	1.333	1.32
CH_3Cl.....	10	1.200	1.20
CH_2Cl_2.....	14	1.142	1.12
$CHCl_3$.....	18	1.111	1.11

The agreement is very good. On the other hand, Capstick's numbers for γ are different, and generally larger than Müller's. They do not fit easily into any similar scheme.

The constant $k = 0.9886$ may be used to calculate the atomic heats of the solid elements. To do this we make the hypothesis that the aggregation in these solids is atomic rather than molecular, and that each atom vibrates in simple harmonic motion about a position of equilibrium. On this assumption each atom will receive, when heated, twice the energy that is taken up by its three translational degrees of freedom. On the principle of equipartition, the energy taken up by one such degree of freedom is the same as that taken up by one translational degree of freedom of the hydrogen molecule, or is equal to the constant k. The atomic heat should therefore equal $6k$, or 5.93. A comparison of this with the observed values of the atomic heats, which range from 5.5 to 6.3, indicates that the hypothesis upon which the result is obtained is in the main correct. It is useless to speculate on the reasons for the different values of the atomic heats of the different elements. We can account for them without denying the doctrine of equipartition by supposing that the vibrations of the atoms are not strictly simple harmonic.

Hitherto we have followed Staigmüller. It seems to me clear, after studying his results, that it is possible to account for the specific heats of gases by supposing the energy distributed equally over certain degrees of freedom; and it seems to me, further, that the explanations which Staigmüller has given of his choices of the number of degrees of freedom which he selects for each type of molecule are at least plausible. The most striking feature of these choices is the small number of internal degrees of freedom assigned to the atoms. It seems as if the atoms were—at least with respect to certain directions of displacement—rigidly bound together. Lord Rayleigh has remarked that, on the ordinary theory of equipartition, no matter how small the play of an atom may be in the sense of a coordinate, the degree of freedom thus indicated must have its full share of energy; so that nothing short of a truly rigid connection in one direction will allow us to neglect the degree of freedom in that direction. From this difficulty we may escape by the use of the distinction

between principal and vibratory degrees of freedom. We may suppose that the forces between the atoms of a molecule vary with exceeding rapidity for relative motions in certain directions, while they do not vary nearly so rapidly for motions in other directions. If this is so, the period of the vibration due to the rapidly varying force will be very small, and the vibration will be of the type which communicates energy rapidly to the ether, so that motion in that sense will correspond to a vibratory degree of freedom. The other motions, of longer period, will correspond to principal degrees of freedom, and will alone be operative in taking up energy when the gas is heated.

It does not seem possible to extend Staigmüller's method directly to other more complicated molecules, for which it would be impossible to assume the atomic arrangement. But if we accept it as verified by its success in describing the specific heats of gases, we may use it to calculate the degrees of freedom in more complicated molecules from their known specific heats, and can then see if the numbers thus obtained appear to have any relation to the numbers and kinds of atoms in the molecules.

To exemplify this mode of procedure let us take the case of methyl alcohol. Its molecular formula is CH_4O; its molecular weight is 32; its specific heat between 5° and 10° is given by Regnault as 0.59; its molecular heat is therefore 19. If we may neglect the potential energy of the molecules with respect to each other in comparison with their kinetic energies, this molecular heat is distributed among 6 degrees of freedom of the molecule and the unknown number of internal degrees of freedom which is to be determined. From Staigmüller's formula, $C_v m = k(a + 2i)$, we get for i, 6.5 +. Of course this is an impossible value for i, which should be integral, but the specific heat of Regnault's specimen of methyl alcohol is almost certainly too high, on account of the presence of water. We may take 6 as the most probable integral value for i. We might speculate about the possible assignment of these degrees of freedom to the different parts of the molecule of methyl alcohol according to its structural formula, but such a procedure would be too arbitrary to carry conviction. What is immediately noticeable is that the number of degrees of freedom thus obtained is equal to the number of atoms in the molecule; so that if we assign one degree of freedom to each atom we can reproduce the molecular heat of methyl alcohol. This would be nothing more than a coincidence if it were not the case that the same relation holds for the other alcohols, whose specific heats are given in Landolt and Börnstein's tables for about 0°.

		No. of Atoms.	i
Methyl alcohol	CH_4O	6	6.5
Ethyl alcohol	C_2H_6O	9	9.5
Propyl alcohol	C_3H_8O	12	12.5
Isobutyl alcohol	$C_4H_{10}O$	15	15
Isoamyl alcohol	$C_5H_{12}O$	18	19

The general tendency of the values of i is to run above the sum of the atoms. This may be taken account of by a special assumption, as, for example, by assigning two degrees of freedom to the hydrogen atom in the hydroxyl group; but it is very likely that the specific heats given in the tables are too high, on account of the diffi-

		No. of Atoms.	i
Ethyl ether	$C_4H_{10}O$	15	15.5
Benzol	C_6H_6	12	10.7
Oil of turpentine	$C_{10}H_{16}$	26	25
Hexan	C_6H_{14}	20	18.5
Heptan	C_7H_{16}	23	21.5
Octan	C_8H_{18}	26	26
Decan	$C_{10}H_{22}$	32	33
Dodecan	$C_{12}H_{26}$	38	40
Tetradecan	$C_{14}H_{30}$	44	46.5
Hexadecan	$C_{16}H_{34}$	50	53
Toluol	C_7H_8	15	13.5

culty of freeing the alcohols entirely from water. The same general rule holds for many other organic compounds.

In the case of several of the organic liquids, the calculated degrees of freedom can be reproduced by assigning 2, or sometimes 3, degrees of fredom to the hydrogen atoms.

		i Estimated.	i Calculated.
Isoamyline.	C_5H_{10}	25	24.5
Acetic acid.....	$C_2H_2O_2$	12	12
Naphthaline	$C_{10}H_8$	26	24.5
Formic acid	CH_2O_2	9	9

And in several compounds containing nitrogen the same rule holds.

		i Estimated.	i Calculated.
Nitrobenzol.....	$C_6H_5O_2N$	19	18.5
Aniline.....	C_6H_7N	21	21
p- and *o*-toluidine.	C_7H_9N	26	26

Other such compounds can not be treated with success so simply.

We may examine a few of the solid organic bodies by the same method. Let us begin with the three pairs of isomers, dextrose and levulose, mannite and dulcite, resorcin (and hydroquinone) and pyrocatechin. The molecular heats of the two members of each of these pairs differ by 6, and we have suggested to us at once that one characteristic difference of two such isomers may be that the molecule in one of them may retain an individuality that the molecule of the other loses, and may vibrate about its position of equilibrium as a whole, so that the value of θ will contain $a = 6$, while that of its isomer will not. If we admit this, we obtain values of the internal degrees of freedom which are satisfied by assigning one degree of freedom to each atom.

		i Estimated.	i Calculated.
Dextrose, levulose.	$C_6H_{12}O_6$	24	25
Mannite, dulcite...	$C_6H_{14}O_6$	26	26
Resorcin, pyrocatechin.....	$C_6H_6O_2$	14	14.3

In the cases of cane sugar and anhydrous milk sugar we can not assume the difference between the molecular heats to be due to the cause previously assigned, but if we examine the structure of these large molecules, we shall find that four of the carbon atoms are differently connected in the molecule from the other eight. If we suppose $a = 0$, and assign 2 degrees of freedom to each of the group of eight carbon atoms in the one case, and to each of the group of four carbon atoms in the other case, we obtain a fair agreement with the calculated numbers of degrees of freedom.

		i Estimated.	i Calculated.
Cane sugar.....	$C_4C_8H_{22}O_{11}$	52	53
Anhydrous milk sugar.....	$C_4C_8H_{22}O_{11}$	49	51

In levulose and dextrose one of the carbon atoms is peculiarly connected with the other constituents of the molecule. If we assign to it 2 degrees of freedom, we obtain an exact agreement with the calculated number of degrees of freedom.

When these organic solids are dissolved in water or alcohol, their apparent molecular heats, that is, the molecular heats calculated for them from the specific heats of their solutions, on the hypothesis that the specific heat of the solvent remains unchanged, are constants for all ordinary concentrations, and differ with the different solvents. These apparent molecular heats are generally greater than those of the same substance in the solid state. It may be that the dissolved molecules have so united themselves with the surrounding water as to weaken the bonds of the water molecules and have thus increased their degrees of freedom; but it is also possible that the degrees of freedom of the dissolved molecules themselves have been increased by the process of solution. If we adopt the latter hypothesis, we may construct schemes apportioning the degrees of free-

dom among the atoms of different sorts which will reproduce the calculated degrees of freedom.

		C	H	O	i Estimated.	i Calculated.
Dextrose	$C_6H_{12}O_6$	1	2	1	36	37
Levulose	$C_6H_{12}O_6$	1	2	2	42	42
Mannite	$C_6H_{14}O_6$	1	2	3	52	51.5
Dulcite	$C_6H_{14}O_6$	1	2	2	46	46
Resorcin	$C_6H_6O_2$	1	3	2	28	29
Pyrocatechin	$C_6H_6O_2$	2	3	2	34	35
Pyrocatechin in alcohol		1	3	1	26	26
Phenol	C_6H_6O	2	3	2	32	33
" in alcohol		1	3	1	25	23

The supposition made before in the case of the sugar molecules, assigning different degrees of freedom to the two groups of carbon atoms, must be made again in this case.

		C_4	C_8	H	O	i Estimated.	i Calculated.
Cane sugar.	$C_4C_8H_{22}O_{11}$	1	2	2	1	75	74
Milk sugar.	$C_4C_8H_{22}O_{11}$	2	1	2	1	71	71
Maltose	$C_4C_8H_{22}O_{11}$	2	1	2	1	71	69

When we turn to the liquids which contain other atoms than those of carbon, hydrogen and oxygen, we find that for a number of them, containing atoms of the chlorine group, the calculated degrees of freedom can be reproduced by assigning one degree of freedom to each atom of carbon and of hydrogen, and three to the remaining atom or atoms.

		i Estimated.	i Calculated.
Ethyl chloride	C_2H_5Cl	10	11
Ethyl bromide	C_2H_5Br	10	9.5
Ethyl iodide	C_2H_5I	10	8.8
Ethyl sulphide	$C_4H_{10}S$	17	17
Chloroform	$CHCl_3$	11	11
Carbon dichloride	C_2Cl_4	14	13.3
Ethylene chloride	$C_2H_4Cl_2$	12	11.3
Ethylene bromide	$C_2H_4Br_2$	12	14
Xylol bichloride	$C_8H_8Cl_2$	22	21.7
Xylol bromide	$C_8H_8Br_2$	22	21
Xylol tetrachloride	$C_8H_8Cl_4$	28	26.5

In the case of the ordinary solid oxides, the general rule is that the calculated degrees of freedom can be reproduced by assigning 3 degrees of freedom to each metallic atom and 1 or 2 to the oxygen atom.

The chlorides and sulphides of the metals conform to the choice of 3 degrees of freedom for the metallic atom, and 3 also for the atoms of chlorine or sulphur.

A question of a peculiar kind and of special interest comes up when we examine the heat capacities of dilute aqueous solutions of electrolytes. Julius Thomsen has shown that these solutions exhibit the very remarkable peculiarity that the apparent molecular heat of the solute diminishes as the dilution increases, so that it even becomes negative after a certain dilution is reached. Now it is evident that this can not be accounted for except by supposing that the water is so associated with some part, at least, of the solute as to have its own heat capacity diminished. The heat capacity is evidently an additive property of the solution. When we think of the solute as associated with some of the water, we may conceive of the solution as made up of the following parts: (1) the water lying outside the groups of water molecules affected by the solute; (2) the undissociated molecules of the solute with the water molecules joined with them; (3) the dissociated ions of the solute with the water molecules joined with them. Each of these parts will have its own heat capacity.

This conception of the composition of a solution leads to a simple formula for its heat capacity, of the form

$$H = W + A(1 - p) + Cp,$$

in which W is the heat capacity of the water used to make up the solution, p is the dissociation factor (it being understood that one gram-molecule of the solute is used in making up the solution) and A and C are constants. The A equals the heat capacity of the undissociated molecule added to the difference between the heat

capacity of the water molecules associated with it and that of the same molecules when free. The C is a similar quantity referring to the dissociated ions.

The formula may most easily be tested in the form $H = W + A + Bp$, when $B = C - A$. We may determine the dissociation factor from Kohlrausch's values of the electrical conductivities. Testing the formula for those solutions for which Thomsen and Kohlrausch furnish sufficient data, we find that it reproduces the observed heat capacities exceedingly well. It breaks down sometimes for concentrations so high as to contain one gram-molecule of the solute to 20 of water.

The formula obtains additional credit from the circumstance that a precisely similar formula gives excellent reproductions of the observed volumes of solutions; these volumes being also additive quantities.

In the cases examined, the constant A is positive, the constant B, negative, and the constant $C = A + B$, also negative.

		A	$-C$
Sodium chloride	NaCl	39	31
Potassium chloride	KCl	97	55
Sodium hydroxide	NaOH	32	33
Potassium hydroxide	KOH	59	51
Ammonium chloride	NH_4Cl	39	23
Hydrochloric acid	HCl	3.6	36
Sulphuric acid	H_2SO_4	37	43
Magnesium sulphate	$MgSO_4$	24	261

If this conception of a solution is admitted, it leads to the view that the element of the solute is associated with several, at least with more than one, molecules of water. This is not in agreement with the hypothesis made by Traube and by Poynting, by which they have explained the laws of osmotic pressure by means of molecular attractions. They suppose that one, and but one, molecule of water is intimately bound with the molecule or the ion of the solute. We can examine this hypothesis by means of the values obtained for C. This constant represents the sum of the heat capacities of the ions of a molecule and of the difference between the heat capacity of the water molecules attached to the ions and their heat capacity when free. We may express this by writing $C = a + \alpha(\phi - s)$, in which a is the heat capacity of the ions, α the number of water molecules in the aggregations around the ions, and ϕ and s the two heat capacities of one water molecule. The values of C are all negative and whether we know the value of a or not, it is certainly positive, and not less than that corresponding to the translational degrees of freedom of the ions, or 6 for a solute that splits into 2 ions. Then $\alpha(\phi - s)$ is surely negative and numerically as great as $C + 6$. If we set $\alpha = 2$, as Traube's and Poynting's theories call for in the case of such solutions as those of sodium chloride and hydrochloric acid, we find, from the values of C, that $\phi - s$ is negative, and numerically equal to or greater than 18. But s is equal to 18, the heat capacity of a gram-molecule of water, and we are led to the conclusion that ϕ is zero or negative; that is, that the heat capacity of the water molecules united with the ions disappears entirely or becomes negative. This result is evidently inadmissible, and I am forced to believe that more than one molecule of water, in all probability several molecules, are associated with each ion.

One serious objection may be raised against this conception, namely, that if it were true the molecular conductivities of all binary electrolytes ought to be nearly the same; for the natural supposition is that the molecular aggregates are pushed by the electric force, and whatever the ions at their centers may be, they all contain about the same number of water molecules, so that they will experience about the same frictional resistance to their motion, and will move at about the same rate. We may evade this objection by considering

the aggregates as unstable, and supposing the ions to slip along through continually changing groups of water molecules. This hardly seems compatible with the considerable interaction which must take place between the ions and the water molecules, to reduce their degrees of freedom as much as the figures indicate; but it is the best way out of the difficulty that I have found.

I may remind you that Jones and his fellow workers, in their study of the freezing points and boiling points of concentrated solutions, have found deviations from the laws of van't Hoff and of Arrhenius, which they have accounted for by the assumption of the formation of such molecular aggregates as I have described.

One is tempted to follow out the suggestions of this hypothesis to see if any general relations can be found in the constants A and C. There are really too many unknown quantities in the expressions for these constants to make it possible to proceed at all except by conjecture, and I have already rioted so much in conjecture, that to go further may seem to carry me beyond the bounds of reason. Yet, if you will indulge me, I will describe such indications of general law as I have detected. Each of the terms A and C is a sum of the heat capacity of part of the solute and of the change in heat capacity experienced by the water molecules when they unite with it. A refers to the molecules, C to the ions. We need a knowledge first of the heat capacities of the molecules and of the ions of the solute. In the case of sulphuric acid, its heat capacity in the liquid state is 34, sufficiently near the value of A, which is 37, to make it possible to suppose that its undissociated molecule either does not associate water molecules to itself or, if it does do so, does not affect their heat capacities. To get the heat capacity of the ions, I am reduced to adding together their atomic heats obtained from the tables. In all but one case where a comparison is possible between this sum and the heat capacity of the solid solute, the two agree well together. The sum for sulphuric acid is 27, agreeing with the heat capacity 26 of the solid acid. Using this number for a in the formula $C = a + \alpha(\phi - s)$, we get $\alpha(\phi - s) = -70$. The symbol α expresses the number of water molecules affected by the presence of each ion, of which there are 3 for each molecule of sulphuric acid. If we venture to suppose that the number of water molecules affected by each ion is 8, the value of α is 24, and $\phi - s = -3$. Now the heat capacity of the water molecules is 18, so that, on these suppositions, the loss of heat capacity of each water molecule due to its association with an ion of the solute is one-sixth its original heat capacity. This would involve a loss either of 3 external degrees of freedom, translational or rotational, or of one external and one internal degree of freedom. I choose the number 8 for the number of molecules of water affected by the ion, because such might be the number of those standing nearest the ion in a regular arrangement which would be compatible with freedom of motion. We may think of the ion as at the center of a cube, with the water molecules at its corners. If the ion reaches out to more distant molecules, the next larger group it will affect will contain 14 molecules.

To a close approximation the values of C and a for hydrochloric acid, ammonium chloride, sodium chloride and sodium hydroxide, lead to the same value -3 for $\phi - s$, on the supposition that $\alpha = 16$, or $8 + 8$. The same result for $\phi - s$ is reached for potassium chloride and potassium hydroxide if we set $\alpha = 22$, or $14 + 8$. In the case of magnesium sulphate the value of C is extraordinarily large, and indicates a much larger group of water molecules around the magnesium ion. That

this element can affect water powerfully is shown from the circumstance that its sulphate crystallizes with seven molecules of water of crystallization.

The values of A are not, except in the cases of hydrochloric acid, magnesium sulphate, and of sulphuric acid, already cited, the same as the heat capacities of the molecules of the solute, but are larger than they are. It is hard to account for this as we did in the corresponding case of the increased heat capacity of the non-electrolytes when they were in solution, by supposing an increase in their own degrees of freedom. We are led rather to suppose that the molecule of solute affects the surrounding water so as to increase its heat capacity. On the assumption that the number of water molecules affected is 8, except in the case of the two compounds containing potassium, and that for them the number is 14, we get in general the value 3 for the change in the heat capacity of each water molecule affected, or an increase of one sixth its heat capacity.

Of course such statements as these are merely suggestions. I hope that in time the specific heats of electrolytic solutions will be so accurately known as to make it possible to feel certain whether or not a law really obtains in the values of the constants of the formula.

Considering the bearing of the relations that have been adduced upon the general question of the equipartition of energy, it seems to me that their general consistency with that principle, especially the way in which the heat capacities of the organic compounds can be portioned out among the atoms by means of simple assumptions about their degrees of freedom, does afford some confirmation of the principle. Mere chance can hardly account for so large a number of successful coincidences.

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TRANSPORTATION AND COMBINATION.[1]

We are so constituted that each of us looks at the problems of life from a somewhat different standpoint. The opinions we form, the principles we uphold, the policies we advocate, are all influenced more or less by the work in which we engage and the kindred range of our reflections. It is natural, therefore, that I should find the origin of many present-day questions in the facts of modern transportation and communication, and that I should entertain views, perhaps indulge in fancies, which those facts suggest.

The primitive man traveled on foot and moved his scanty belongings by carrying them in his arms or on his back. Even the rude vehicles and water-craft which he eventually learned to construct were propelled by his own muscle, and we can only guess how long it was before he obtained any other motive power for the transfer of his person or his property. In every way his life was meager and isolated, for he had not acquired the art of writing, and intercourse with his fellows was confined to ordinary speech. Outside the family to which he belonged, or the tribe with which he gathered, he had no community of interest, felt no friendship and desired no alliance. His associations were as limited as his means of conveyance.

In a later but still very remote period there came a great increase of motive power by the subjugation of animals, and their employment for transportation on land, and by the use of sails and rudders which multiplied many times the efficiency of water carriage. When these two results were secured, man had added to his own bodily powers the superior strength of beasts of burden and the enormous energy

[1] Address of the vice-president and chairman of Section I—Social and Economic Science, American Association for the Advancement of Science, New Orleans, 1905.

derived from the winds of heaven. This was an immense advance and marked the beginning of that wonderful civilization which slowly followed. The animal kingdom was brought into service for the various functions of land distribution, and the ship which could be sailed and guided made every waterway subservient to man's requirements. Early in this period also he learned to express his ideas by symbols or written words, and thus was enabled to transmit his thoughts by the same agencies that transported his possessions.

This leads to a fact of history which seems to me not merely significant, but profoundly impressive. With the subjection of animals and the use of wind-propelled vessels, both of which achievements reached a high degree of perfection in the unknown past, the means of transportation, broadly speaking, remained unchanged and unaugmented until almost down to the present time. Long before other agencies of conveyance were dreamed of, while ox and horse, oar and sail, were the only means of transport, the race had occupied most of the habitable globe and reached high levels of national greatness. Strong governments were established, vast populations engaged in varied pursuits, and opulent cities crowded with every luxury. The institutions of society had acquired strength and permanence, the arts of leisure and refinement had approached the limits of perfection, and inductive science had laid firm grasp on the secrets of nature. Great inventions and discoveries had widened the fields of activity, furnished the means and incentive for multiplied vocations, and opened up in every direction alluring vistas of advancement. In a word, there was the developed and splendid civilization of only little more than threescore years ago, before any new or different motive power was utilized for production or distribution.

To my mind it is a matter of fascinating import that the long procession of progress down to the century just ended was conditioned by and dependent upon agencies of transportation which were themselves essentially unprogressive and incapable of important betterment. True, there were minor modifications from time to time in the line of mechanical adjustment, but the general methods employed, and the results obtained, showed no marked improvement or material alteration from those applied in the earliest days of commerce. Reduced to the forms in ordinary use, there were at the last as at the first the beast of burden on land and the oar and sail on water. Yet thus hampered and restricted in the means of transportation, which is the basis of all development, there was built up in the long process of years the varied and advanced civilization which the last century inherited.

Then all at once, as it were, into and through this social and industrial structure, so highly organized, so complex in character, so vast in its ramifications, yet so adjusted and adapted to the fixed limitations of animal power, was thrust the new mode of conveyance by mechanical force, the sudden wonder of transportation by steam. The advent of this new and marvelous agency was the greatest and most transforming event in the history of mankind. It wrought an immediate and radical change in the elemental need of society, the means of distribution. The primary function was altered both in essence and in relations. The conditions of commercial intercourse were abruptly and fundamentally altered, and a veritable new world of energy and opportunity invited the conquest of the race.

No other triumph over the forces of nature compares with this in its influence upon human environment. It has directly and powerfully affected the direction and volume of commercial currents, the location

and movements of population, the occupations and pursuits in which the masses of men are engaged, the division of labor, the conditions under which wealth is accumulated, the social and industrial habits of the world, all the surroundings and characteristics of the associated life of to-day. The world has seen no change so sudden and so amazing.

The next fact to be noted is hardly less remarkable. Not only are the new methods of transportation incomparably superior in speed, cheapness and capacity, but, unlike those which have been supplanted, these new methods are themselves capable of indefinite increase and expansion. The maximum efficiency of an animal is so well known as to amount to a constant quantity, and this unit of power is virtually unchangeable. Substantially the same thing is true of a vessel of given dimensions and given spread of canvas. For this reason distribution remained, as I have said, the one fixed and inflexible element to which all other activities, however elastic and progressive, were necessarily adjusted and by which they were limited.

Now, a special and most suggestive feature of transportation by steam, electricity and other kinds of mechanical force is that its capacity is not only unmeasured and unknown, but will doubtless prove to be practically inexhaustible. That is to say, no certain limits can be assigned to the operation or effect of these new agencies as compared with those which have been superseded. Therefore, speed may reach many times the rate now attained, the size of vehicles may be greatly increased and the cost of carriage for the longest distances reduced to an astonishing minimum; so that as progress goes on in developing the means and methods of distribution, the habits and needs of men will be more and more modified, with consequences to social order and the general conditions of life which may be far greater than have yet been imagined.

But this is not all. Another fact is still more wonderful. Coincident with this sudden transfer from animal power to steam have come the new and amazing means of transmitting intelligence. In a brief generation the barriers of time and distance, hitherto so formidable, have been swept away by telegraph and telephone. No longer limited to the agencies by which material things are transported, we send our thought and speech with lightning swiftness to the four quarters of the globe, and hold all lands and peoples within the sphere of instant intercourse. So recent is this miracle that we are still dazzled by its marvels without realizing its tremendous import.

That this substitution of steam and electricity as the instruments of commerce and communication has been an immeasurable gain is witnessed here and everywhere by half a century of unparalleled progress. Along these wondrous pathways the world has literally leaped. Released from dependence on beasts of burden, the entire realm of industry has been quickened and enlarged; productive energy has been vivified by new and limitless means of distribution; the products of the whole earth are embraced in wide circles of exchange; all the luxuries of all lands are brought to every household; wealth has multiplied until we are almost surfeited with its abundance, when other people possess it; the genius of invention has been stimulated to larger exercise, the sphere of thought grandly extended, the impulses of charity awakened to nobler activity, while keener sympathy through closer contact is pointing the road to real brotherhood.

But these manifold benefits have not been secured without many and serious evils. The potent energy which produced such prodigies of utility and convenience has

generated an array of forces which already test with severe strain the structure of modern society. So radical a change in the methods of distribution, and consequently of production, was sure to be attended with peril as well as beneficence, and to entail a series of results, immense and far-reaching. Passing by those acute abuses which are incident to the process of development, which are transitory and must gradually disappear, we may well consider the more profound and permanent effects, what I venture to call the economic effects, of present and future methods of transportation and intercourse upon the whole range of social activity. This brings into view again the impressive fact mentioned at the outset, and suggests some graver consequences than those that appear on the surface and appeal to ordinary observation.

When movement was measured by the strength and endurance of animals, only a limited area could be reached from a common center. Its slowness and expense confined all inland distribution within narrow bounds. Only eighty years ago it took a week to send a letter, and cost $125 to move a ton of freight, from Philadelphia to Pittsburg; and the average price for carrying the necessaries of life was not less than twenty cents a ton for each mile of distance. On such a basis most commodities were shut off from distant markets, and farm products, for example, would seldom permit of conveyance more than 100 or at most 150 miles. Only such articles as were of small bulk and weight compared with their value were moved to any considerable distance from the place of production. For this reason the requirements of an ordinary family were almost wholly supplied from near-by sources. And this means—without amplifying the statement—that productive energy, for the most part, was restricted by the consuming capacity of the surrounding neighborhood. The forces outside each little circle were but feebly felt and had slight influence upon its separate affairs. Broadly speaking, the business of each locality was adjusted to its own conditions and was practically undisturbed by like operations in other places. What we call competition was held in check by slow and costly means of conveyance; its effects were moderate and limited, its friction seldom severe.

But the use of steam for motive power and electricity for sending news increased enormously the range of accessible markets, and at once intensified competition by the celerity and cheapness of distribution. Industrial strife has already become world-wide in extent and distance an ineffectual barrier against its destructive assaults. For distance as a commercial factor is not at all a matter of miles, it is merely a question of time and money. So the effect of cheap conveyance and quick communication is to bring remote places closer together. For all the practical needs or enjoyments of life Manila is nearer New York now than Montreal was a century ago; and the whole world could be easier governed from Washington to-day than could the United States when the capital was located on the Potomac.

Our grandparents got their supplies mainly in the neighborhood where they resided, and only a few persons were concerned in their production. To-day it may safely be said that five millions of people and five hundred millions of capital are directly or indirectly employed in furnishing a family dinner. When merchandise of every description is moved by the ton at great speed from one end of the land to the other, and at an average cost of less than three quarters of a cent a mile, as is now the case, the expense of transport is but a trifling impediment to the widest distribution.

Nor should we forget that it was the

opening up of new and ever enlarging markets, by the cheapness of steam transportation, which gave the first opportunity for the extensive use of machinery; and this in turn quadrupled the capacity of labor and greatly reduced the cost of large scale production. By this revolution in the methods of manufacture—caused by the railroad and steamship—the mechanic was supplanted by the operative, and the skilled and independent craftsman of former days found his occupation gone. For what chance now have hand-made articles when the factory-made product is carried across the continent at nominal cost? But the factory without the railroad would be only a toy shop. If its wares had to be hauled over country roads by mules and horses, the points they could reach would be few and near by, and thus contracted sales would limit the size of the plant and the volume of its business. It is simply because transportation is now so speedy, so cheap and so abundant that great establishments have become profitable and driven their smaller rivals from the field.

These facts—which might be multiplied without limit—bear directly, as I think, and with a force not fully perceived, upon the whole problem of industrial competition. The argument runs this way: As the means by which industrial products are distributed become more convenient, quicker in action and less expensive, the area of distribution rapidly enlarges, and as the area of distribution enlarges the competition of industrial forces increases in something like geometrical ratio. The movement of property by rail in the United States alone already exceeds four millions of tons every twenty-four hours. Think of the rivalry of products, the strife of labor, the strain and struggle of trade, which such a movement implies. With the constant acceleration of that movement, which is certain to happen, how long can the friction be endured? How soon will it become unbearable?

The truth is that new conditions have arisen and new methods must be adopted. All the pressure of modern life impels to the coordination of effort. We see that discord and antagonism, to say nothing of their moral bearing, have far less efficiency than harmony and cooperation. The world is searching for economies. It is intolerant of needless expense. The way a thing can be done the easiest and cheapest is the way it is bound to be done and the way it ought to be done. We want the best results and find that they come from combination. The old aphorism, 'in union there is strength,' takes on a new meaning. It is the law of growth and increase. It applies to industries as well as to individuals. To unite is to advance. The concentration of process is the expansion of output.

Thus the potent agencies by which distribution is now so rapidly and cheaply effected, which so combine and intensify the forces of production, are fast altering the conditions and changing the character of industrial development. And the end is not yet; it outruns imagination. What will be the ultimate effect of these methods of conveyance and intercourse when brought to higher perfection and employed with still greater efficiency? When these agencies of commerce are increased in number and capacity, as they will be; when cost is still further and greatly reduced, as it will be; when speed is doubled, as it will be, and quadrupled, as it may be; when the whole United States shall have reached the density of population now existing in Great Britain—how can industrial competition possibly survive?

When Adam Smith wrote 'The Wealth of Nations,' it took two weeks to haul a wagon-load of goods from London to Edinburgh, and such a thing as a business or industrial corporation was virtually un-

known. To-day the great enterprises of the world are in the hands of corporations, and the time is fast approaching when they will absorb all important undertakings. Why? Simply because the railroad and the steamship—cheap and rapid transportation, all the while growing cheaper and quicker—ever widening the area of profitable distribution, furnish the opportunity, otherwise lacking, for the employment of larger and still larger capital. This opportunity permits and encourages the concentration of financial resources; so that, within limits not yet ascertained, the larger the business the greater its possibilities of gain. But the legitimate, the inevitable offspring of corporations is monopoly. Why? Simply because the operation of these massive forces—reaching and opposing in every market of the world—begets an extremity of mutual danger which always invites and often compels a common agreement as to prices and productions; that is, a trust. Just as the implements of warfare may become so devastating in their effects that nations will be forced to live in amity, so the destructiveness and exhaustion of commercial strife in these larger spheres of action will make combination a necessity.

So, in the measureless and transforming effects of modern transportation, and the ends to which it resistlessly tends, I find the primary cause of the economic revolution upon which we have entered. The incoming of these new and unfettered forces not only changed the basic function of society, but greatly disturbed its industrial order. In the effort to restore a working equilibrium strange questions arise and novel dfficulties are encountered. Already we are compelled to doubt the infallibility of many inherited precepts and to reopen many controversies which our grandsires regarded as finally settled. The ponderous engine that moves twice a thousand tons across an empire of states, the ocean steamer that carries the population of a village on its decks and the products of a township in its hold, the vast mergers of producing and distributing machinery whose colossal grasp covers land and sea, are indeed splendid evidences of constructive genius and financial daring, but more than this, they are economic and social problems whose complexity bewilders and whose magnitude dismays. They force us to discredit the venerable maxim that 'competition is the life of trade,' and warn us I think, that the political economy of the future must be built on a nobler hypothesis. If it be true in the long run, as I believe experience teaches, that where combination is possible competition is impossible, is it not equally true that combination becomes possible just in proportion as transportation becomes ampler, speedier and cheaper? So the opportunity, if not the necessity, for combination has already come in many lines of activity and will certainly come in many more. For the circumstances that permit competition, its *sine qua non*, is mainly difference of conditions. Practically speaking, this difference is chiefly found in the means of distribution. As that difference disappears, with the constantly diminishing time and cost of transport, the ability to combine will increase and the inducement to do so become overwhelming. That seems to me the obvious tendency of industrial and social movements to-day, and that tendency, I predict, will be more and more marked as time goes on.

How fast the process will develop, or what phases it will assume, does not yet admit of confident forecast. Many experiments will be tried, many failures occur, before the readjustment is accomplished. Remedies will be sought in profit-sharing, in the distribution of corporate stocks among employees, in the socialization of public utilities, in largely increasing the

functions of government. By whatsoever road reached the ultimate if not early outcome will probably be some form of centralized control with diffused or decentralized ownership. Meanwhile, the exactions of monopoly, the feebleness of legal restraints, the heaping up of fabulous fortunes, the prejudice of the ignorant, the envy of the incapable; and through all and over all the inappeasable voice of labor demanding, not without reason, a larger share of the wealth which it produces.

That these great consolidations are wholly desirable I certainly do not pretend. On the contrary, they occasion much cause for regret and not a little for grave apprehension. The utilization of new forces, the transfer to new methods, the control of producing and distributing agencies by huge combinations, must in the nature of the case inflict many hardships and involve many surrenders. But a great principle underlies this movement, the principle of industrial peace and efficiency, the principle of cooperation. Beside all question that principle is to govern, despite all drawbacks its operation will be beneficent.

So, in the unrest and discontent around us, deep-seated and alarming here and there, I read the desperate attempt to avoid the effects of industrial competition and a tremendous protest against its savage reprisals. Every trust and combination, whether organized by capitalists or by artisans, every strike and lockout, is a repudiation of its teachings and a denial of its pretensions. The competitive theory may have answered the age of mules and sailboats and spinning-wheels, but it fails to satisfy the interlacing needs, or to sustain the interdependent activities, which are founded on modern methods of intercourse and distribution; it is a theory unsuited to the era of railways and wireless telegraphy, this era of ours, so restless in thought, so resistless in action.

I much mistake, therefore, if we are not entering upon a period of great transitions, a period of difficulty and many dangers. The whole structure of industry and social life is liable to be subjected to a strain—possibly to a shock—for which experience furnishes no guiding precedent. We have settled the administrative questions; we can collect taxes, build courthouses and pay the policeman. We have settled the political questions; the nation lives and will live, the greatest and grandest in all the earth. But the further test is now to come, the test of the ocean liner and the limited express. Can we settle the economic questions? Can we raise this wide realm of industry from selfishness to charity, from strife to friendship, from competition to cooperation, from the warring instincts of the savage state to the larger and nobler needs of associated life? This is the problem of railroad and steamship, of telegraph and telephone, of the subtle and limitless forces of modern life, the problem which will test the wisdom of statesmanship and tax the resources of public authority.

Martin A. Knapp.

SCIENTIFIC BOOKS.

A Systematic Handbook of Volumetric Analysis. By Francis Sutton, F.I.C., F.C.S., etc. Ninth Edition, revised and enlarged. Philadelphia, P. Blakiston, Son & Co.

Sutton's 'Analysis' is so well known that the highest praise that can be paid this book is the statement that it is even better than the eighth edition. But few new chapters have been added, the most important being on magnesium and the azo-dyes. The individual chapters have been but little changed. Few new methods have been added and fewer obsolete processes dropped. Conservatism is undoubtedly necessary in a work of this kind, but it may be carried too far. The book would be more valuable if the author with his large experience were more ready to discard

old methods and adopt new ones. The English journals are referred to almost entirely throughout the book and one gets the impression that the continental periodicals have been neglected. The table of contents now contains a list of the tables throughout the book. This will be found useful and will save time. On the whole this book is certainly the best of its kind in the English language and it will always occupy an important place on the table of the working chemist. F. L.

A Text-book of Quantitative Chemical Analysis. By J. C. OLSEN, A.M., Ph.D., Professor of Analytical Chemistry in the Polytechnic Institute of Brooklyn, formerly Fellow of the Johns Hopkins University. Pp. xix + 513. New York, D. Van Nostrand & Co.

As stated in the preface, Professor Olsen has not attempted to produce a reference book for experienced analysts, but rather a book for college students. Accordingly he has devoted considerable space to theory and the explanation of the various steps in an analysis and has followed the sequence in which he presents the subject to his classes. The first chapters treat of 'General Operations and the Determination of Water,' then follow chapters on 'Gravimetric Analysis of Metals, Acids, Alloys and Minerals,' 'Electrolytic Methods,' 'Volumetric Analysis,' with special chapters on 'Oxidation and Reduction' and 'Precipitation Methods,' 'Technical, Water, Oil, Fat and Gas Analysis' and finally 'Stoichiometry,' and tables. There is not such a mass of facts as in Fresenius and yet enough methods are given for the teacher to choose good and interesting work for everybody. The last few chapters are particularly important, introducing as they do technical methods into the ordinary course. It is to be regretted that the author has not given in the first chapters a fuller discussion of the application of the ionic theory. Though apparently less essential, it is as necessary to the correct understanding of ordinary gravimetric methods as to those that are purely electrolytic. Perhaps the author assumes that no student is now allowed to take up quantitative analysis until he understands the ionic theory, but a review will do no harm. The directions are clearly given throughout and the student is made to feel that there is a reason for every step. The author tries to do away with the ridiculous practise of calculating results far beyond any significant figure but goes to the other extreme when he tells the student: "If an analysis is carried out by a process or for a purpose in which an error of one per cent. may be present no pains need be taken to secure greater accuracy than this in any step of the process." If this were logically carried out and there were ten steps in the process a loss of one per cent. in each step would certainly not give a result within one per cent. The student can be taught to work with all possible care and accuracy without attributing too small an error to his results. A chapter on stoichiometry should not be necessary in a work of this kind. Students with a proper understanding of the atomic theory and the simplest mathematics should be able to make all necessary calculations. It has evidently been the experience of the author, as of all teachers of the subject, that a majority of students need special training in stoichiometry. There is a fundamental fault somewhere in the student's course either in chemistry or in mathematics. Would it not be well for the teacher to force the student to work out all these problems by himself without any aid but the atomic theory and the rule of three? The reviewer hopes that Professor Olsen will omit the chapter on stoichiometry from the next edition of his book and he feels that there will soon be a second edition of so good a work.

F. L.

SCIENTIFIC JOURNALS AND ARTICLES.

THE leading article in the *Journal of Nervous and Mental Disease* for January is a study by Dr. H. C. Gordinier of two unusual brain tumors, one a multiple cylindroma of the base of the brain, the other a neuro-epithelioma of the choroid plexus of the fourth ventricle. This is followed by Dr. Onuf, of Craig Colony, with a study of a number of cases of epilepsy presenting partly muscular atrophies, partly defective muscular action

with clearly demonstrable atrophy, with definite distribution of these disturbances. The paper is clearly illustrated from photographs of the patients. Dr. Morton Prince contributes a short paper on a case of multiform tic including automatic speech and purposive movements which was presented by him at the meeting of the Boston Society of Psychiatry and Neurology, March 16, 1905. This issue includes reports of the New York Neurological Society for October 3, 1905, and of the Boston Society of Psychiatry and Neurology for October 19, 1905.

SOCIETIES AND ACADEMIES.

THE AMERICAN MYCOLOGICAL SOCIETY.

THE society held its third annual meeting in connection with the American Association for the Advancement of Science at New Orleans, January 1, 1906.

In the absence of the president, Professor Charles H. Peck, the vice-president, Professor F. S. Earle, presided.

The new constitution recommended by the committees of the Botanical Society of America, the Society for Plant Morphology and Physiology and the American Mycological Society, as a basis for the union of the three societies, was adopted and the present officers continued as a committee with power to cooperate in the completion of the details of reorganization.

The following papers were presented at the meeting:

J. C. ARTHUR: 'Some Reasons for Desiring a Better Classification of the Uredinales.'

S. M. TRACY: 'Uredineæ of the Gulf States.'

W. G. FARLOW: 'Some Peculiar Fungi New to America.'

F. S. EARLE: 'North American Gill Fungi.'

BRUCE FINK: 'Lichens and Recent Conceptions of Species.' (Read by title.)

E. M. FREEMAN: 'The Affinities of the Fungus of *Lolium temulentum*.'

C. L. SHEAR: '*Peridermium cerebrum* Peck, and *Cronartium Quercuum* (Berkeley).'

C. L. SHEAR: '*Ramularia:* An Illustration of the Present Practise in Mycological Nomenclature.'

P. H. ROLFS: 'Notes on Cultures of *Colletotrichum* and *Glœosporium*.'

PERLEY SPAULDING: 'The Occurrence of *Fusoma parasiticum* Tubeuf in this Country.'

P. H. ROLFS: 'Notes on *Pachyma cocos*.'

P. H. ROLFS: '*Penicillium glaucum* on Pineapple Fruit.'

C. L. SHEAR,
Secretary-Treasurer.

THE BIOLOGICAL SOCIETY OF WASHINGTON.

THE 26th annual and 408th regular meeting was held on December 23, 1905, and the following officers were elected for the ensuing year:

President—F. H. Knowlton.

Vice-Presidents—T. S. Palmer, W. P. Hay, E. L. Greene and E. W. Nelson.

Recording Secretary—M. C. Marsh.

Corresponding Secretary—W. H. Osgood.

Treasurer—David White.

Councilors—A. D. Hopkins, J. N. Rose, A. K. Fisher, L. Stejneger and A. B. Baker.

President Knowlton was nominated for vice-president of the Washington Academy of Sciences.

THE 409th regular meeting was held on January 6, 1906.

Mr. J. W. Titcomb exhibited a mud nest weighing thirteen pounds, of the red ovenbird or hornero (*Furnarius rufus*) from Argentina, South America.

Dr. L. O. Howard gave an account of the symposium on 'Yellow Fever and Other Insect-borne Diseases' held under the auspices of the section on physiology and experimental medicine at the recent New Orleans meeting of the American Association for the Advancement of Science.

Mr. Henry Van Deman exhibited two fine specimens of winter apples from the Hood River Valley, Oregon. These represent two eastern varieties, the Newtown and Esopus, which have gained markedly in excellence by transplantation to the Pacific slope.

The paper of the evening was presented by Mr. Alvin Seale, 'Notes on the Natural History of the South Pacific Islands.' He gave an account of the general features of several groups of islands visited, with descriptions of the characteristics of the native populations. His remarks were well illustrated by a series of lantern slides.

M. C. MARSH,
Recording Secretary.

THE WELLESLEY COLLEGE SCIENCE CLUB.

AT the November meeting of the Wellesley College Science Club, Dr. J. C. Bell presented a paper on the 'Reactions of the Crayfish to Sensory Stimuli.'

His experiments show that the animals react negatively to white light of different intensities in the proportion of two to one. Difference of intensity, within the limits used, causes no change in the proportion. Increase of temperature slightly increases the proportion. Green, yellow and blue, when compared with white light, show only a slightly greater number of reactions, but in the combination red-white the red has 73 per cent. of the reactions.

Experiments upon the chemical sense, which are still in progress, go to show that the animals are sensitive to chemical stimuli over the whole surface of the body, but particularly upon the anterior appendages. There is no evidence whatever of hearing, and the animals depend chiefly upon touch for the seizure of food.

GRACE E. DAVIS,
Secretary.

THE BERKELEY FOLK-LORE CLUB.

THE second regular meeting of the club during 1905–6 was held in the Faculty Club of the University of California, on Tuesday evening, November 28. President Lange called the meeting to order.

The minutes of the last meeting were read and approved. The following new members were elected: Professor H. A. Overstreet, Mr. A. H. Allen and Professor W. F. Bade.

Professor F. B. Dresslar read a paper on 'Some Studies in Superstition,' based on superstitions known to and in part credited by advanced school students on the Pacific coast. Special attention was paid to the degree of credence given to superstitions. Particular attention was also given by the speaker to the subject of mental preference for odd numbers. At its conclusion Professor Dresslar's paper was discussed by the members.

The meeting was adjourned.

A. L. KROEBER,
Secretary.

THE CHEMICAL SOCIETY OF ST. LOUIS.

THE St. Louis Chemical Society held its usual monthly meeting on January 8. Mr. H. E. Wiedemann presented a paper entitled 'The By-products of the Packing House.' Special emphasis was laid on the successful work of the chemist, which has transformed the waste-heap of former days into a large number of useful products.

C. J. BORGMAYER,
Secretary.

DISCUSSION AND CORRESPONDENCE.

PLEASE EXCUSE THE KELEP.

TO THE EDITOR OF SCIENCE: It is naturally a deep disappointment to learn from a recent number of SCIENCE that my efforts to elucidate the habits of the kelep have fallen so far short of the high ideals of entomological literature which Professor William Morton Wheeler advocates. This is largely my own fault, no doubt, as Professor Wheeler seems to intimate. But with sufficient scolding one must needs improve. The devotion of so many columns of SCIENCE to this missionary effort is certainly to be appreciated. That Professor Wheeler has felt it worth while to resume his admonitions constitutes also a welcome assurance that the future no longer appears altogether hopeless, for the last of his previous instalments closed with a despairing vow of eternal silence 'until the Greek Kalends,' or something to that effect, if I remember correctly.

Nevertheless, my efforts are largely foredoomed to failure, on account of the very backward and unscientific habits of my insect pets. Unlike true, civilized ants, they have not learned the gentle art of regurgitation, but persist in going about with large, round drops of nectar on their bills. They regularly carry it into their nests in this way, and feed it to their friends and families without having once swallowed it, or spewed it up again. This incredible conduct is very easy of observation. It has been witnessed by a dozen or more of my colleagues, and I have no doubt that Professor Wheeler will be able to verify it whenever he has time to undertake an investigation of the subject.

In the matter of breeding habits, too, there is no longer a hope of meeting Professor Wheeler's wishes. The queens seem never to leave the nest voluntarily, or to fly about, as ants should. Instead of raising an annual brood of sexual individuals, there are young males and females in the nests at all seasons of the year. Males were seen at many different times going about freely in the kelep-protected cotton fields of Guatemala, and were sometimes captured by workers and taken down into their nests. Copulation inside the nest has been observed by Mr. Argyle McLachlan at Victoria.

It is needless to multiply such fatal discrepancies between the habits of the kelep and those ascribed to the insects which Professor Wheeler has studied. There remains only to beg for mercy for these misguided creatures, and for one who is made to suffer so much embarrassment for having placed on record facts which do not coincide with 'the literature of the subject.'

My mistake with *Leptogenys* was especially stupid and careless, for, as Professor Wheeler reminds us, he had already published, in three different papers, the statement that the queens are indistinguishable from the workers, except by their distended abdomens. But to me they appear to be distinctly larger insects, of a distinctly more reddish color, resembling in these respects the queens of the keleps. Can it be that Professor Wheeler had only laying workers of *Leptogenys,* and not true queens? Such a possibility might naturally suggest itself, but one must hesitate to believe that Professor Wheeler could repeat for a fourth time a statement which might prove so easily to be erroneous. Of course, one would not make such minor discrepancies the basis of a general criticism of Professor Wheeler's excellent work on Poneridæ and ants; it is mentioned now only because his own reference to it appears somewhat inconsiderate.

If one were to generalize on this series of entomological episodes the deduction would be that adequate ignorance of literature is a necessary qualification for learning the habits of a new insect like the kelep, for at each important step the investigation has been met by Professor Wheeler's *non possumus.* Last year he was quite as unable to believe that the keleps would kill boll-weevils as he is to credit now their failure to regurgitate nectar. After surviving so many of these literary dangers it is only natural that one become a little reckless, and venture even to hope that in the course of another year the additional facts, at present so objectionable, will receive due credence, having now become a part of 'literature of the subject.'

Regarding the classification of the poneroid insects, Professor Wheeler can be entirely reassured. The kelep does indeed have some habits comparable with those of honey-bees, but these traits have not seemed to require its removal from its systematic position next to the Poneridæ. The several families of unexpectedly diverse insects which have been included hitherto in the Poneridæ still constitute a natural group, of higher systematic rank, coordinate on the one hand with the drivers, and on the other with the series of families which may be referred to still as 'true ants,' though these may prove to be an artificial assemblage, their phylogenies not having been traced. The differences of social organization appear to forbid a close alliance of the true ants with the poneroid series, or a derivation from them. Ants and keleps are similar, it is true, as all hymenoptera are, but many of the resemblances prove to be superficial and indicative of parallel development rather than of any recent community of origin. Biological history abounds in such instances where groups popularly supposed to be closely alike have been found to be essentially different.

To permit the new facts and modifications of doctrine to be properly assimilated, it is now highly desirable that peace be restored in the happy family of entomology. If I promise to be very careful to write future papers on the kelep as well as ignorance and other limitations permit, it may be that Professor Wheeler will deem it safe to renew and abide by his former pledge of silence; not, indeed, till any too remote a period like the Greek Kalends, but perhaps until he has seen and studied some living keleps, or at least some

other members of the family Ectatommidæ. The mistakes he would then discover would be, I realize, still more embarrassing than those he has detected at so much longer range, but the discussion of them might have a correspondingly greater scientific value. And yet it may be that even in this I am still begging the question or asking an unfair advantage, for increasing knowledge often sets cruel limits in the free fields of literary sport.

O. F. COOK.

VICTORIA, TEXAS,
December 16, 1905.

SPECIAL ARTICLES.

A NEW THEORY OF SEX-PRODUCTION.[1]

THE last volume of the *Proceedings* of the German Zoological Society contains an interesting address by Professor Richard Hertwig in which is developed a new theory of sex-production based on his long-continued experiments on protozoa and applied to the interpretation of the results of new experiments by himself on amphibia and those of his pupils Issakowitsch and von Malsen on daphnids (*Simocephalus*) and on *Dinophilus*. Professor Hertwig's conclusions demand especial attention, since they are on the whole antagonistic to the view, which has been widely accepted in recent years, that sex is already determined in the fertilized egg, though he does not deny that such early determination may exist in some cases.

The new experimental results brought forward are as follows: The work of Issakowitsch (since published in full in the *Biologisches Centralblatt*) proves that in *Simocephalus* sex-production shows a definite reaction to temperature changes. At 24° C. a parthenogenetic production of females, with only the occasional appearance of a male, continues until the culture dies out; while a reduction to 16° quickly leads, and reduction to 8° immediately leads, to the appearance of males and later (sometimes immediately) to the production of winter eggs. Issakowitsch also shows that a similar effect may be produced by starvation without change of temperature, and hence he concludes (like Nussbaum in the case of rotifers) that the change of temperature probably acts indirectly through its effect on nutrition. Von Malsen's work on *Dinophilus* shows that in a culture maintained for months at 10°–12° C. male and female eggs are produced in a ratio of 1:3, while at 25° the ratio rises to 1:1.75, and sometimes reaches 1:1. Both these cases seem free from the objection that applies to so many of the earlier experiments on sex-modification that the statistical results may be vitiated by different rates of mortality in larvæ of different sexes. Unfortunately the same can not be said of Professor Hertwig's own experiments on frogs, ingenious and interesting as they are. Experiments with changes of temperature gave no really satisfactory result, though Hertwig seems inclined to believe that higher temperatures favor the production of females. On the other hand, either over-ripeness or under-ripeness of the eggs (a condition obtained by artificially delaying or hastening fertilization) led in every case to a large excess of males. Like those of earlier observers, these results are not very convincing, owing to the high mortality of the larvæ, which must be reared to the time of metamorphosis before the sex can certainly be determined. The most satisfactory results appear to have been obtained from a culture of over-ripe eggs in which 20 per cent. to 30 per cent. of the fertilized eggs were reared to this period, the result being 317 males to 13 females; and in one case of under-ripe eggs 40 larvæ that were successfully reared were all males. These results can hardly be ascribed to accident; but the dubious character of the statistical data obtained by rearing tadpoles and other larvæ through long periods of time has been so clearly shown by the experience of many other observers that the true interpretation of the facts in this case seems by no means clear.

Hertwig's general theory of sex-production was primarily suggested by his own earlier experiments on the relation between nucleus and protoplasm in the protozoa. These experiments led him to the conclusion that the

[1] R. Hertwig, 'Ueber das Problem der sexuellen Differenzierung,' in *Verhandlungen der Deutschen Zoologischen Gesellschaft*, 1905.

ratio between the nuclear and the protoplasmic mass (the 'Kernplasmarelation,' expressed by the formula k/p) tends towards a normal value that is in the long run constant for the species, though it undergoes cyclical changes both in the individual cell and in successive generations of cells. In ordinary or 'functional' growth the value of k/p undergoes a temporary decrease, owing to the more rapid growth of the protoplasm, but this induces a subsequent more rapid 'divisional' growth of the nucleus which raises the value of k/p to a point above the normal, and finally leads to cell-division by which the normal ratio is again approximately restored. Long-continued 'autogenous' (*i. e.*, vegetative or asexual) reproduction causes, however, a gradual permanent increase in the value of k/p (*i. e.*, a nuclear hypertrophy) and this necessitates a reorganization by conjugation through which the normal condition is restored. The value of k/p may also be experimentally altered by conditions of food, temperature and the like; and here, according to Hertwig's view, lies the possibility of affecting the sexual relations by external conditions.

Professor Hertwig's application of this conception to sex-production in the metazoa is, we think, open to serious criticism. His central assumption is that the 'Kernplasmarelation' differs in the two sexes, having a higher value in the male (*i. e.*, the nuclear mass is assumed to be relatively greater in that sex), and any influence that tends to increase this value, whether in the gametes, in the zygote, or in the developing embryo, favors or determines the production of the male condition, and *vice versa*. Since the egg contributes tc the germ the entire mass of protoplasm and half the nucleus, while the spermatozoon contributes only half the nucleus, 'the egg naturally takes the lion's share' in the determination of the 'Kernplasmarelation' and hence in the determination of sex. In case of the frog, over-ripe and under-ripe eggs tend alike to produce males, because in the former (long retained in the oviduct) the nuclear substance has increased at the expense of the protoplasm, while in the latter the protoplasm has not yet completed its growth—either case giving a relatively high value to k/p. In view of the fact that the nucleus breaks down and the first polar spindle is formed at the time the egg leaves the ovary, this explanation does not seem very convincing, at least in the case of over-ripe eggs. As applied to the cases of *Simocephalus* and *Dinophilus* the argument becomes too involved for detailed review here, since the two cases are diametrically opposed, a higher temperature favoring in the one a continued production of parthenogenetic females, and in the other the production of males; but here too Hertwig attempts to show that an explanation may be found in the assumption of alterations in the value of k/p directly or indirectly traceable to the effect of temperature.

It is somewhat surprising to find on how small a basis of actual fact the central assumption of the hypothesis rests. No new cytological evidence is brought forward, and the only facts given in direct support of the assumption are, first, that the value of k/p is enormously greater in the spermatozoon than in the egg, and secondly, that in all cases (such as *Dinophilus*) where male and female eggs are distinguishable before fertilization the former are smaller than the latter. It is difficult to see how the first of these facts bears on the problem, for the question is how the male or female value of k/p is produced in the *zygote*, which results from the fusion of one gamete from each sex whether it produces a male or a female. The second fact is assumed by Hertwig to mean that the male-producing eggs have a relatively high value of k/p, owing to a deficiency of protoplasm. 'Nach allem, was wir über Befruchtung wissen, müssen die Kerne dieser Kleineier (of *Dinophilus*) ebenso gross sein, wie die der Grosseier' (p. 196). But Korschelt's figures of *Dinophilus* show the nuclei of the small eggs very much smaller than those of the large ones, in the ovaries, in the new-laid eggs within the capsules, and in the early and late cleavage-stages, and there is no actual evidence, either in these eggs or in any of the other cases, that the value of k/p is greater in the males than in the females. On the contrary, in the only known cases of nuclear dif-

ference between the sexes (the protoplasmic mass being the same as far as can be observed) it is the female, not the male, that has the larger quantity of nuclear material. This fact has recently been demonstrated by the reviewer in a dozen different species of hemiptera, representing eight genera, and the structure of the spermatozoa shows that the same is undoubtedly true of many other species. The difference is here one of nuclear constitution and is irrespective of temporary changes of nuclear volume such as are common to all cells. But even here we can not regard the quantitative difference of the nuclei as being primarily responsible for the sexual differentiation, for in some of the species of the same group no perceptible difference in this respect exists between the sexes.

Without questioning the interest and value of Professor Hertwig's experimental results, it, therefore, seems to us that his theory of sex-production is without real foundation, and that, in the specific form that he has given it, it is untenable. E. B. WILSON.

ASTRONOMICAL NOTES.

THE FIGURE OF THE SUN.

Two articles by Professor Charles Lane Poor have recently appeared in *The Astrophysical Journal* dealing with a variation in the figure of the sun. This research comes with special interest at the present time, when so many writers are trying to trace relationships between various terrestrial phenomena and different forms of solar activity.

Among the remarkable photographic work done by Rutherfurd was a series of photographs of the sun. These are still of such unimpaired excellence that they permit admirable determinations of the sun's diameter. From a series of such plates extending over the years 1870–1872, measurements were made of the polar and equatorial radii, from a discussion of which it appears that the polar radius is sometimes greater and sometimes less than the equatorial radius. The individual determinations of this difference (polar radius —equatorial radius) vary between $+0.77''$ and $-0.72''$, and the means are as follows: for 1870, September 22, $+0.50'' \pm 0.10''$, for 1871, July 19, $-0.32'' \pm 0.16''$, and for 1872, July 2, $+0.22'' \pm 0.09''$. There is thus indicated a change in the relative values of the polar and equatorial diameters of the sun. This conclusion, if true, is of great importance, and it is not strange that Professor Poor desired to verify his results by reference to other and independent determinations of the form of the sun.

A large number of heliometer measurements of the diameter of the sun were available from a very thorough discussion, by Dr. Auwers, of the transits of Venus, in 1874 and 1882. Dr. Auwers reached the conclusion that the diameter of the sun at distance unity is 1,919.26″, and that the polar diameter slightly exceeds the equatorial diameter. This difference, however, was attributed by him to the personal equation on the part of the observers between measures of vertical and horizontal diameters. The observations as discussed by Auwers gave no indication of a variation in the relative values of the different diameters, but were rearranged in a form suitable for this discussion by Professor Poor.

Measurements were also made of a short series of photographs of the sun taken at Northfield by Dr. Wilson. Both the heliometer determinations and the Northfield photographs seem to confirm the results obtained from the Rutherfurd photographs. Professor Poor thus sums up his conclusions:

> The present investigation would seem to show, therefore, that the ratio between the polar and equatorial radii of the sun is variable, and that the period of this variability is the same as the sun-spot period. The sun appears to be a vibrating body whose equatorial diameter, on the average, slightly exceeds the polar diameter. At times, however, the polar diameter becomes equal to and even greater than the equatorial—the sun thus passing from an oblate to a prolate spheroid.

In a second paper Professor Poor extended his investigations to include the elaborate heliometer determinations of the sun's dimensions, carried on at Göttingen by Shur and Ambronn. These observations covered a full sun-spot period, from 1890–1902. Ambronn, who discussed the observations, gave his special attention to the mean diameter of the sun, so that again it became necessary to

rediscuss the results. This Professor Poor has done. His conclusion is that these observations, when properly interpreted, also confirm his theory of the variable form of the sun, and curves are given which confirm his views.

It should be stated, however, that the differences derived from the Göttingen observations are extremely small, and that Ambronn was of the opinion that the differences between his determinations of the polar and equatorial diameters were due to accidental errors.

RELATION BETWEEN THE MOTION IN THE LINE OF SIGHT, AND THE VARIATION IN BRIGHTNESS OF VARIABLE STARS.

The discovery of new variable stars has gone on with increasing rapidity in recent years, until now about three thousand variables are known. The majority of these have been found by photographic means at the Harvard Observatory. Fair progress has been made also in the study of the light-curves of these stars, and definitive elements have been found for several hundred of them. It can not be said, however, that great progress has been made in the determination of the underlying causes which produce the variations. In the case of the Algol stars it is sure, both from theoretical considerations, and from spectroscopic determinations of their motion, that the variation is caused by a relatively dark, eclipsing body. The brilliant lines of incandescent hydrogen, which appear near maximum in the spectra of many stars of long period indicate with considerable certainty that the variations in their light are associated with eruptive disturbances of some sort. It was long ago pointed out that our sun is probably a variable star of long period and small range, and many variable stars may exist, whose changes are caused by spots of greater or less size. Nevertheless, it still remains true that for the great majority of variable stars no sure key has been found to the secret of their changes.

Two recent bulletins of the Lick Observatory (Nos. 62 and 83) have dealt with a new and extremely important phase of this question. In these, Dr. Ralph H. Curtiss, of that observatory, shows that for the well-known variable star W Sagittarii there is a most intimate relation between the velocity of motion in the line of sight and the changes in brightness. This was well indicated by a comparison of the Lick determinations of motion with the early observations of the light-curve by Schmidt. The similarity is most strikingly shown, however, when the velocity-curve is compared directly with the light-curve derived from recent photometric measurements by Professor Pickering, given in the Harvard Annals, Vol. 46, Part 2. A comparison of these curves shows a close resemblance even in the details, and proves conclusively that both phenomena are due to the same underlying causes. Incidentally a striking proof is furnished of the accuracy of the two widely separated investigations thus critically compared. Dr. Curtiss's work marks a distinct step in advance in the study of variable stars, and it is to be hoped that the research may be extended to as many and as faint variables as possible.

S. I. Bailey.

CURRENT NOTES ON METEOROLOGY.

BRIEF COMMENT ON RECENT ARTICLES.

In the recent numbers of the *Meteorologische Zeitschrift* (Nos. 7–10, Vol. XXII., 1905) there have been many contributions of general scientific interest, on which the following brief comments are made:

The exploration of the free air at great heights has been giving records of very low temperatures. On March 2, last, at a height of 9,717 meters, the temperature was —85.4° C., and on April 4, at 11,010 meters, it was —79.6° C. These records were obtained by means of *ballons-sondes* sent up from Vienna. So rapid has been the accumulation of data from the free air that the mean annual temperature and the vertical temperature gradients at each interval of one kilometer up to a height of eleven kilometers have been determined, using the results obtained on nearly 600 balloon ascents.

Antarctic meteorology is making rapid progress. On the Swedish expedition, under

Otto Nordenskjöld, observations were continued for twenty months, from March, 1902, to October, 1903, on Snow Hill Island (lat. 64° 22′ S.), and for seven months on Paulet Island (lat. 63° 22′ S.). The low mean annual temperature, 10.8° F., resulted from the low summer mean of 28.2°, this being the lowest Antarctic summer mean on record with the exception of that of the *Discovery* expedition. The minimum was —42.5° F.; the maximum, 48.7° F. The maximum temperatures occurred in spring and fall. The prevailing wind was southwest, and wind velocities showed no striking relation to pressure changes. Rain fell occasionally. The cirrus clouds moved from between west and west-southwest, and the same direction was noted in the intermediate clouds. The winter was clearer than the summer, when fogs prevailed.

THE "Magnetic and Meteorological Observations made by the 'Southern Cross' Antarctic Expedition, 1898–1900," under the direction of C. E. Borchgrevink (London, 1903), include the records made at Cape Adare from March 3, 1899, to January 28, 1900, the first winter records from the Antarctic continent (lat. 71° 18′ S.; long. 170° 9′ E.). Anticyclonic winds from east-southeast to south prevailed (41 per cent.); calms were noted nearly half of the time (41 per cent.). The curious fact was noted that the lowest pressure sometimes occurred with the *end* of a storm.

THE laws governing the size of rain-drops have received some attention, notably of late at the hands of Defant, of Innsbruck, who reaches the following conclusion. The formation of drops depends upon the combination of smaller droplets, but, contrary to the views of Reynolds and Lenard, this union is between droplets of the same size, or of nearly the same size. Drops of unequal size unite less easily the greater the difference in their sizes.

As the result of actinometrical measurements made on Mont Blanc in August and September, 1904, Hansky finds the most probable value of the solar constant 3.23 calories.

Das Gewitter, by Albert Gockel, 2d edition, Köln, 1905, is an excellent 'popular' discussion of present knowledge concerning thunderstorms, including lightning, lightning rods and atmospheric electricity in general.

CIRRUS clouds, on account of their delicacy and beauty, offer an attractive field for individual non-instrumental study, which may lead to interesting conclusions regarding the various methods of formation of these clouds. Osthoff, of Cologne, has recently made a considerable investigation of this sort, which will prove interesting to any one undertaking a similar quest.

THE transparency of fog has been experimentally investigated by Haecker, of Kiel, by means of a new method depending on exact photometric measurements of the visibility of surfaces at different distances. The instrument used is a 'polarization-photometer.' A practical result of such work would be the application of the results to lighthouses, ships' lights, etc.

A REMARKABLE 'dust fog,' observed in the Malay Archipelago in October, 1902, of such density as to interfere with navigation, has been investigated by the Batavia Observatory staff, large numbers of circular letters of inquiry having been sent to ship captains whose business took them to those seas at the time in question. The causes of this remarkable dust fog have been sought in the deficient rainfall of the year 1902; in extended forest fires, especially in Borneo and southern Sumatra, and in the transportation of dust by the southeast trade from Australia. As the progress of the dust from Australia could be followed, by successive stages, northward, the latter cause was doubtless the most important one.

MEINARDUS of Berlin has been paying special attention to the relation between the general winds, the circulation of the water in the North Atlantic Ocean and the weather of adjacent lands. His latest conclusions are summarized as follows, A and B being groups of conditions which occur in association with one another.

A. 1. Weak Atlantic circulation (August–February).

2. Low water temperatures on the European coast (November–April).

3. Low air temperatures in central Europe from February to April.

4. Little ice off Newfoundland in spring.

5. Heavy ice off Iceland in spring.

6. Bad wheat and rye harvests in western Europe and northern Germany.

B. 1. Strong Atlantic circulation (August–February).

2. High water temperatures on the European coast (November–April).

3. High air temperatures in central Europe from February to April.

4. Heavy ice off Newfoundland in spring.

5. Little ice off Iceland in spring.

6. Good wheat and rye harvests in western Europe and northern Germany.

The wind observations made during the Antarctic expedition of the *Gauss* show that the station was on the poleward side of the barometric depression which surrounds the Antarctic ice. There were few winds from the western quadrant, and an increase of pressure to the south, with an anticyclone in that direction, must be assumed. The station was, on the whole, nearer the circumpolar low-pressure ring than the anticyclone. Cyclonic weather was more common than anticyclonic. Low temperatures prevailed with westerly winds and during calms. Easterly winds brought a rise of temperature.

Meteorological observations during the solar eclipse of August 30, last, made at Bernau, in southern Germany, showed that the temperature fell from 65.8° to 59.7° in ten minutes, and then rose again. The wind fell from a moderate velocity to a calm during the eclipse, and then increased again.

R. DeC. Ward.

NOTES ON THE HISTORY OF NATURAL SCIENCE.

HIPPOCRATEAN FISHES.

Included in the *Corpus hippocraticum* is, next after Herodotus, one of the oldest of Greek prose writings, a work 'On Regimen,' in four books, by an unknown author, yet regarded by Galen as not unworthy of the 'father of medicine' himself. Throughout all antiquity, this work, especially the second book, was held in high esteem; nor can its interest be said to have vanished at the present day, whether regarded from a historical, literary or purely scientific standpoint. In that part of the second book which treats of the dietetic value of various plants and animals, as many as fifty-two species of the latter are enumerated, seventeen of which are fishes; and their order of arrangement is such as to have suggested to Burckardt[1] the idea of a definite system, called by him the 'Coan scheme of classification.'

Notwithstanding the large number of fishes mentioned in this work, some of the names occurring here for the first time, I have been unable to find any reference to it in ichthyological literature. Both Littré and Fuchs, in their translation of the text—there is no English version—attempt a precise identification of species, but judged by the standard set by Hoffman and Jordan in their 'Catalogue of Greek Fishes,' it can not be said that these classicists have been uniformly successful. A comparison with the catalogue referred to shows that at least ten of the Hippocratean species can be recognized with certainty, five are doubtful, and the remaining two may be despaired of as hopeless. One of these, ἐλεφιτίς, also written ἐλεφητίς, seems to be peculiar to the work in question, and no one has ventured a conjecture as to its meaning.

Of great importance for the early history of ichthyology are the abundant notices contained in Athenæus, 90 species of fishes being enumerated by him in alphabetical order. The extent to which this author drew upon Dorion's compendium, and the sources from which this in turn was derived, have been set forth in an extremely interesting essay by Wellmann.[2] From this we take the following estimate of Dorion's treatise, citations from the latter occurring in thirty-four passages of Athenæus:

Die erhaltenen Fragmente zeigen, dass das Werk in ziemlich umfassender Weise die Fischwelt be-

[1] 'Das koische Tiersystem,' *Verh. Naturf. Ges. Basel*, XV., pp. 377–414, 1904.

[2] *Hermes*, Vol. XXIII., pp. 179–193 (1888). Other valuable references to the early literature are given in the chapter contributed by Eugene Oder ('Ueber Fische und Fischfang') to Susemihl's 'History of Alexandrian Literature,' Vol. I., 1891.

handelte und nicht blos über die verschiedenen Namen eines und desselben Fisches und deren Schreibung, über die verschiedenen Arten, deren Unterschiede und Aufenthaltsorte Aufschlüsse gab, sondern auch auf Vorschriften über Kochen und Braten derselben einging.

THE REAL UNICORN.

IN his review of Dr. Murray's recent work on museums, Mr. F. A. Bather[1] observes that the author refrains from any attempt to decide what the unicorn really was, notwithstanding that numerous endeavors have been made to identify fabulous creatures with modern quadrupeds. It would have been very agreeable had Mr. Bather chosen himself to enlighten us on this matter; since he does not, the following note is suggested.

What appears to have been the origin of the 'real unicorn,' that is to say, of the creature made known to the western world under that name by Ctesias, has been set forth in several interesting essays by German writers, amongst whom it will be sufficient to mention Schräder, Lüders, Lauchert and Goldstaub, the two last-named concerning themselves especially with the history of the 'Physiologus.'[2] Excellent reason is shown by these authors for freeing Ctesias of the charge of deliberate invention; he is believed to have recorded things pretty nearly as he saw them; no attempt is evident on his part to impose upon the credulity of others; although proved to be mistaken in some particulars, it is possible for us to discover the reason, the unicorn furnishing a case in point.

One can readily see that the description of the 'Monoceros' which we owe to Ctesias does not repose upon living specimens, any more than does that given by Herodotus of the Phœnix; what the former actually saw, and correctly depicts, are animal reliefs graven upon the walls of the Persian court at Persepolis, the like of which exist to this day. Among these representations the figure of the unicorn is several times repeated, being, in fact, conventionalized profiles of an Asiatic ruminant new to the Greeks, with the two horns appearing in side-view as one. Excellent copies of these figures are to be found in standard works on ancient Persian and Assyrian art.

The post-classical history of the unicorn, together with the whole menagerie of folklore, has been a favorite study of French writers, the important works of Berger de Xivrey,[3] le P. Cahier, Hippeau and others leaving little further to be desired in their line. In particular these authors have traced the extent to which popular natural history traditions became modified, early in the Christian era, through the influence of moral and religious interpretations. Thereafter, the stream of popular ideas relating to animals divides into two parallel branches, which remain for many centuries distinct: the one manifesting itself in the numerous versions of the Bestiary, the other in that purely fabulous natural history which gained wide circulation under the title of 'Wonders of India,' and whose source appears to have been a forgery of a letter from Alexander to Aristotle concerning the Indian conquest. Despite its unauthenticity, a prototype of the fraudulent document in question would seem to have been current as early as the Alexandrian period.

C. R. EASTMAN.

[1] *Museumskunde*, Vol. I. (1905), p. 170.

[2] Lauchert, F., 'Geschichte des Physiologus' (Strasburg, 1889). Goldstaub, M., 'Der Physiologus und seine Weiterbildung.' *Philol.*, Supplement, Bd. VIII. (1901), pp. 337–404.

[3] Traditions Tératologiques (Paris, 1836).

ROBERT BOWNE WARDER.

ROBERT BOWNE WARDER died at his home in Washington, July 23, 1905, after an illness extending over nearly a year.

Professor Warder was born in Cincinnati, O., March 28, 1848, and spent his early life in his country home at 'Aston,' North Bend, O. His character was formed under the influence of the Society of Friends, and this faith remained the dominant feature of his life. From childhood he showed the effect of his parents' training and example, in a broad and catholic view of the ethics of life, and in a love of truth and scientific investigation. This devotion to truth was an especial characteristic and governed his life and actions throughout.

He was graduated from a Friends' institution, Earlham College, at Richmond, Ind., in 1866, and afterwards spent some time at the Illinois State University at Champaign, where he was instructor in chemistry and natural philosophy. This work of teaching seemed to show Professor Warder his natural bent, and his energy was thenceforth devoted to studying the broad principles underlying all natural science. He spent some years in traveling, chiefly in the western half of the United States, in connection with the different state geological surveys. In 1873 he went to Harvard, where he was graduated as B.S. in chemistry in 1874.

After graduating at Harvard he spent a year traveling in Germany, studying at Giessen under Heinrich Will, and at Berlin under Hofmann. His attention was, however, especially devoted to methods of teaching chemistry in the German universities, and the application of theoretical chemistry to the practical sciences. His chief aim was to fit himself in the broadest sense for his work of teaching. This was his main desire throughout life, to help others, and he never faltered. On returning to this country he was associated with Professor F. W. Clarke at the University of Cincinnati from 1875 to 1879 as professor of chemistry and physics. Professor Warder early saw the close relation between these then distinct branches of natural science, and his papers on 'The Speed of Saponification of Ethyl Acetate' and 'Evidence of Atomic Motion within Liquid Molecules' were pioneer investigations in the field of the physical chemistry of to-day.

He was engaged in this line of research from 1879 to 1883, when he accepted the chair of chemistry at Purdue University, where he remained until 1887. This position carried with it the duties of state chemist, work of a commercial character rather foreign to his natural tastes, but to which he gave the same painstaking devotion that characterized all his work. Even these routine analyses were made to pay tribute to physical chemistry, as is shown by papers on 'Influence of Time in Fertilizer Analysis,' 'Speed of Dissociation of Brass,' etc.

In 1884 he married Gulielma M. Dorland, who also belonged to the Society of Friends, and like himself was interested in evangelical work. Their life together was one of perfect harmony.

It was probably about this time that Professor Warder felt more keenly than ever the call to help others in another field than chemistry. His philanthropic and evangelical work had always been foremost in his mind and labors, and in 1887 he accepted the professorship of chemistry at Howard University in Washington. Here he labored until he died, teaching chemistry and physics, but above all setting an example and teaching the principles of a Christian life with an unselfish devotion.

In spite of lack of facilities, his work at this period on 'Dynamical Theory of Albumenoid Ammonia,' 'Recent Theories of Geometric Isomerism,' 'Cross Fertilization of the Sciences' and 'The Major Premise in Physical Chemistry' showed his natural inclination to this phase of chemistry.

Professor Warder's later papers were chiefly devoted to applying the laws of mass action to and showing the speed of chemical reactions for the analytical data obtained by other investigators.

He was essentially a critic and his devotion to truth caused him to scrutinize the investigations of others with the same zealous care with which he looked for flaws in his own work. This high standard, coupled with an unusual modesty, often caused a hesitation which sometimes obscured his really profound knowledge.

No one went to Professor Warder for [illegible] and was turned away empty-handed. What he had was given freely, and he seemed to feel that no labor was too great in his fundament[illegible] desire to help others.

At a meeting of the Washington Section of the American Chemical Society, held on November 9, 1905, the following resolutions were adopted:

Whereas, death has removed from earth our friend and co-worker, Robert Bowne Warder.

Resolved, That in his death chemistry has lost a disciple who gave to her service the enthusiasm

of his youth, the strength of his manhood and the counsel of his riper years.

The American Chemical Society has lost a member who through both his experimental researches and his theoretical studies and especially by his now classical work on reaction velocities has made unusual contributions to the advancement of the science for the promotion of which this society exists. These distinguished services have placed all who are interested in chemistry under lasting obligations, and his name will be honored so long as this science is cultivated.

The student of chemistry has lost a friend who was always ready to extend a helping hand and to contribute freely from his rare store of knowledge and extended experience.

The community has lost a man who by his civic virtues, his high ideals, his willingness to assume and faithfulness to perform duties of an unusually trying kind, his catholicity of views and of interests, and his tolerance of and kindly sympathy for the opinions of others commanded the respect and admiration of all with whom he came in contact.

His life was a benefaction, his presence a blessing, his practical christianity a continual source of edification and his career one of great usefulness to man.

We ask that this tribute to his memory be spread upon the minutes of the society; that it be printed in the proceedings and in SCIENCE, and that a copy be forwarded to Mrs. Warder.

On behalf of the society,

FRANK V. CAMERON,
F. W. CLARKE,
WM. H. SEAMAN,
FREDERIC P. DEWEY.

SCIENTIFIC NOTES AND NEWS.

THE German emperor has conferred on Professor Simon Newcomb the order 'pour le mérite' in science and the arts.

PROFESSOR EMIL FISCHER, of Berlin, has been elected president of the German Chemical Society.

THE Technical Institute at Munich has conferred the honorary degree of doctor of engineering on Dr. Felix Klein, professor of mathematics at Göttingen.

THE Geological Society of London has made the following awards: The Wollaston medal to Dr. Henry Woodward, formerly keeper of the geological department of the British Museum; the Murchison medal to Mr. C. T. Clough, of the Geological Survey, known for his excellent work in Scotland; the Lyell medal to Professor F. D. Adams, the Canadian geologist, whose petrographical work is well known; the Prestwich medal to Mr. William Whitaker, whose long labors on the Tertiary deposits of England render him a most fitting recipient. The funds are awarded as follows: The Wollaston to Dr. F. L. Kitchin, who, not long ago, was appointed paleontologist to the Geological Survey of Great Britain; the Murchison to Mr. Herbert Lapworth, who has followed in his father's footsteps with excellent work on the Welsh border; the Lyell is divided between Mr. W. G. Fearnsides and Mr. R. H. Solly; the Barlow-Jameson goes to Mr. H. C. Beesley.

DR. A. B. RENDLE, who has been an assistant in the botanical department of the British Museum since 1888, has been appointed keeper of that department in succession to Mr. George Murray, recently retired.

PROFESSOR T. J. J. SEE, U. S. Navy, has recently been elected to membership in the Société française de Physique, Société astronomique de France, Circolo Mathematico di Palermo, and to life membership in the Astronomical Society of the Pacific.

MR. G. W. ROLFE, instructor in sugar analysis in the Massachusetts Institute of Technology, has been given leave of absence for several months in order that he may go to Porto Rico to take charge of a sugar plantation on the Constancia estate.

PROFESSOR FRANCIS E. LLOYD, of the department of biology of Teachers College, Columbia University, has resigned to accept a position in connection with the Desert Laboratory of the Carnegie Institution.

IN accordance with the terms of the fund established anonymously, a course of eight lectures will be delivered by Harvard professors at Yale, following the first lecture under the fund delivered by President Eliot on November 13, on the subject, 'Resemblances and Differences among the American Universities,' and printed in this journal. The remainder of the series will be on philosophy.

Professor Palmer will give seven lectures on 'Some Aspects of Ethics' and Professor Münsterberg will give one lecture.

Professor Wilhelm Ostwald, of the University of Leipzig, has completed his courses at Harvard University, and has since been giving a course of lectures on 'The Relations of Energy to Life and Thought' before the psychological department of Columbia University, and a course of lectures on 'Physical Chemistry' before the chemical department. Professor Ostwald will return to Germany next week.

The sixth lecture in the Harvey Society course was given by Professor Lewellys F. Barker, of Johns Hopkins University, at the New York Academy of Medicine on January 27 on 'The Neurones.'

Professor J. J. Thomson lectured before the Royal Institution on January 19 on 'Some Applications of the Theory of Electric Discharge to Spectroscopy,' and on February 2 Professor S. P. Thompson lectured on 'The Electric Production of Nitrates from the Atmosphere.'

It is said that a library building to be erected on the campus of the University of Chicago is to be the memorial of the late President Harper. It will be erected on the Midway Plaisance, between Lexington and Ellis Avenues, and the cost is to be defrayed by popular subscription.

It is planned to present to the city of Philadelphia a statue of Dr. Joseph Leidy, to be erected in the City Hall Plaza. Dr. Leidy, who was born in that city in 1823 and died there in 1891, added much to its scientific eminence, and as president of the Academy of Natural Sciences, professor of human and comparative anatomy and zoology in the University of Pennsylvania, and president of the Wagner Free Institute of Science, accomplished much for these institutions. The sum of $10,000 is being collected for the memorial. Contributions may be sent to Mr. Edward B. Smith, treasurer of the Leidy Memorial Committee, 511 Chestnut Street, Philadelphia, Pa.

We learn from *The British Medical Journal* that in connection with the centenary of the birth of Joseph Skoda the Vienna Medical Society has decided to issue a complete edition of the famous physician's writings. A committee has been formed for the purpose of carrying this scheme into execution; it consists of Professors Chrobak, von Schrötter, von Neusser, Benedikt and Heitler, with Professors Neuburger and Töply as representatives of the historical sciences. In this edition will be collected Skoda's lectures in the University of Vienna, the articles contributed by him to medical journals, and the special scientific opinions and judgments often delivered by him, together with the other published works of the great clinician.

We learn from *Nature* that a memorial to the late Dr. George Salmon, F.R.S., provost of Trinity College, Dublin, was unveiled on Friday, January 5, in the national cathedral of St. Patrick's, with which Dr. Salmon was officially associated during the best years of his life. An account of the ceremony appeared in the *Kensington Express* of January 5, from which we learn that the memorial consists of two windows in St. Peter's Chapel, the work of Mr. C. E. Kempe, depicting scenes in the career of St. Peter, and a medallion of Dr. Salmon, by Mr. A. Bruce-Joy, with a Latin inscription of which the following is a translation: "That the name of George Salmon may abide in the memory of mankind this monument has been erected by his faithful friends and grateful pupils. Fellow of Trinity College, Dublin—afterwards regius professor of divinity, and finally provost, he was for thirty-three years chancellor of this cathedral church. A mathematician both adroit and powerful, he probed with keen insight the beginnings of christian history, and specially the origin of the New Testament Books; as teacher and councillor he was unwearied in the service of the Irish church. Shrewd, courteous, serious, kindly. He was born in 1819, and died in 1904. The fear of the Lord is the distinction of wisdom, and before honor is humility."

Dr. H. J. P. Sprengel, F.R.S., the inventor of the mercury air-pump, died on January 14, aged seventy-two years.

THE deaths are announced of Professor H. Ravl-Rückhard, docent in anatomy at Berlin, and of Dr. Chelius, professor of geology in the Technical School at Darmstadt.

BY the will of Marshall Field, filed on January 24 in Chicago, the city receives $8,000,000 for the endowment and maintenance of the Field Columbian Museum, now situated in Jackson Park. The bequest is on condition that within six years from the death of Mr. Field there shall be provided a satisfactory site for the permanent home of the museum.

BY the will of W. C. Putnam, the Davenport (Iowa) Academy of Sciences becomes prospectively one of the most richly endowed institutions of its kind in the world. Mr. Putnam left an estate of $700,000 with provisions for limited incomes to relatives, the remainder of the revenues to be paid the academy and the entire estate to go to that institution at the death of the surviving brothers and sisters. His art collection and library, each the most valuable private collection in the state, are left to the academy, with provision for a fireproof building in which they are to be installed.

AT its annual meeting on January 24 the board of regents of the Smithsonian Institution adopted a resolution accepting the offer of Mr. Charles L. Freer, of Detroit, to convey to the institution the title to his art collection, and to bequeath $500,000 for the construction of a fireproof building in which to house it. Mr. Freer is to make the conveyance at once, although the collection is to remain in his possession until his death.

BY the will of the late Charles H. Nelson, of Grafton, Mass., his home and $75,000 is to be given to the city of Grafton for a library, and $10,000 is to be given to Vassar College, both bequests to take effect after the death of his widow.

BY the will of the late Dr. John C. Warren, professor of anatomy and surgery at Harvard University, who died in 1856, the most perfect mastodon skeleton in the world and many other specimens of great value are likely to pass into new hands. Dr. Warren wrote a still classical monograph on the 'Mastodon Giganteus of North America' in 1852, in which the skeleton in question was described. He put it into a private museum of his own in company with many other curious specimens. There is the great 'Shawangunk' head, which is that of a still larger mastodon. There are many non-articulated bones of the Baltimore mastodon. The very fine collection of fossil foot-prints may be mentioned, but the reader is referred for further details to the *Harvard Bulletin* of January 17, 1906. Dr. Warren left this collection to be held by his children in trust as long as any should survive, after which it is to be shared by the living grandchildren. The time has now come for the latter to consider what disposition they will make of it. Professor J. Collins Warren and Professor Thomas Dwight, both of the Harvard Medical School, are among the heirs.

THE Royal Botanic Society, London, has received £1,000 from Dr. Robert Barnes, £200 from Lord Lister and other gifts.

THE department of mammalogy of the American Museum of Natural History received in December a series of eight hippopotamus skulls showing various stages of growth from the young to the adult from Lake Ngami, South Africa. The department has also secured four huge giraffe skulls from Bechuana Land, South Africa. Comparison with the skull of the museum's mounted giraffe skeleton shows that these newly acquired skulls must have belonged to animals 18 feet high.

THE government of Brazil has decreed a prize of $10,000 for any one who exhibits 100,000 Maniçoba rubber trees within 18 months from the date of the announcement, and three other prizes for the three next largest plantations, the smallest of which, in order to gain a prize, must not be of less than 20,000 trees.

WE learn from the London *Times* that the first expedition sent out to West Africa by the Liverpool Institute of Commercial Research in the Tropics left the Mersey on January 6 by the Elder-Dempster steamer *Zungeru*. The members, who are conducted by Lord Mountmorres, director of the insti-

tute, are—as chemist, Mr. Kenneth Fisher, senior demy of Magdalen College, Oxford, who has been for some time engaged on research work at Jena University; as botanist, Mr. L. Farmer, assistant curator of herbarium at Kew; as entomologist, Dr. Slater Jackson, of McGill University, and formerly curator of the Canadian government biological station; and as commercial adviser, Mr. Coates, a trader who has long acted as buyer on the West Coast of Africa for Mr. John Holt, one of the best-known of African merchants. The expedition is proceeding to Dakar, Bathurst, Konakey, and, if possible, to the Cameroons. Being only an experimental expedition, the stay on the West Coast will not be of very long duration; in fact, Lord Mountmorres is to return in time to visit the exhibition of rubber at Ceylon in April. But should the results prove satisfactory, there is every probability that the institute will despatch a second expedition to spend a long period in Africa. Sir Alfred Jones (president of the institute) and many leading Liverpool gentlemen were present on board the *Zungeru,* and gave the members of the expedition a most cordial send-off.

UNIVERSITY AND EDUCATIONAL NEWS.

Announcement is made that Mr. John D. Rockefeller has given $1,450,000 to the University of Chicago. Of this sum, $1,000,000 is for the permanent endowment, $350,000 to cover the current expenditures or deficit of the various departments of the university to July 1, 1907, and the remaining $100,000 is to provide a fund, the interest of which is to go to the widow of the late President Harper during her lifetime.

Brown University will build a library as a memorial to John Hay. Mr. Andrew Carnegie has consented to give one half of the cost, which is estimated at $300,000.

Mr. John D. Rockefeller has given $115,000 to Acadia College, at Wolfville, N. S., a Baptist institution.

A new building is to be erected immediately at the Worcester Polytechnic Institute to be devoted to the department of electrical engineering. For some time past the quarters have been inadequate for the work, and the increasing size of the entering classes for the past three years has finally resulted in the decision on the part of the trustees to proceed immediately with the erection of this new building. Last fall a course in electric railway engineering was added. In the erection of the new building it is proposed to provide ample facilities for the course of instruction in this work and also to introduce the most complete experimental facilities possible. The quarters which have in the past been occupied by the electrical engineering department will be divided between the departments of physics and of chemistry, thus affording to each of these departments much needed relief.

Messrs. Mallinckrodt, of St. Louis, offer $500 for the year 1906–07 to a student of chemistry in the Graduate School of Harvard University, on condition that he serve the subsequent year in the Mallinckrodt chemical works at a suitable salary.

By the will of the late Sir J. S. Burdon-Sanderson, formerly regius professor of medicine at Oxford, the laboratory of the pathological department of the university is bequeathed the sum of £2,000, as an endowment to provide for pathological research there, the fund to be vested in the professors of human anatomy, physiology and pathology, who are to have absolute discretion as to the application of the fund.

The widow and children of the late Dr. von Siegle, of Stuttgart, have, as we learn from *Nature,* presented 50,000 Marks in memory of the deceased to the chemical institute of the University of Tübingen.

Dr. Louis Cobbett has been appointed professor of pathology, and Mr. L. T. O'Shea, professor of applied chemistry, in the University of Sheffield.

Mr. Charles S. Bradley, practising electrician of New York City, known for his contributions to electricity and chemistry, has been elected acting professor of chemical practise in the Carnegie Technical Schools, Pittsburg.